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CONTENTS

FURTHER MEASUREMENTS OF THE EFFECT OF PRESSURE ON THE ELECTRICAL RESISTANCE OF GERMANIUM

By P. W. Bridgman

MISCELLANEOUS MEASUREMENTS OF THE EFFECT OF PRESSURE ON ELECTRICAL RESISTANCE

By P. W. Bridgman

THE EFFECT OF PRESSURE ON SEVERAL PROPERTIES OF THE ALLOYS OF BISMUTH-TIN AND OF BISMUTH-CADMIUM

By P. W. Bridgman

FURTHER MEASUREMENTS OF THE EFFECT OF PRESSURE ON THE ELECTRICAL RESISTANCE OF GERMANIUM

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CONTENTS

INTRODUCTION

In two recent papers[1] I have presented measurements of the effect of pressure on the resistance of germanium: in the first paper the pressure range was 30,000 kg/cm² with a true liquid as a pressure transmitting medium; in the second paper the pressure range was 100,000 kg/cm² and the transmitting medium silver chloride. In both cases the measurements were made at room temperature, this being necessitated by instrumental limitations. It was recognized that it would be desirable if measurements could be extended to temperatures in the intrinsic range. This paper presents such an extension, but at the cost of a considerable reduction in the pressure range. Apparatus was at hand which had been previously used[2] in resistance measurements to low temperatures. In this apparatus pressure was transmitted by nitrogen, the use of a gas having been necessitated in order to avoid the freezing under pressure which would have occurred if pressure had been transmitted by a liquid. Nitrogen gas is also adapted to be the medium at high temperatures, avoiding now difficulties arising from the chemical decomposition or other activity of a transmitting liquid.

The former apparatus was used with no essential modification. This apparatus consists of an upper cylinder, maintained at room temperature, in which is the manganin pressure gauge and the in-

sulating plugs connected to the specimen to be measured. The upper cylinder is connected with a tube to the lower cylinder, in which is mounted the specimen to be measured, the connections to which are taken out through the connecting tube to the insulating plugs in the upper cylinder. The potentiometer method of measuring resistance was used, necessitating four leads for the specimen. The lower cylinder is kept at the desired temperature and thermostatically controlled. The temperature range was from 0°C to 200°C, the latter being above the intrinsic temperature of any of the specimens. The pressure range was kept at 7,000 kg/cm² in the interests of safety, the pipe connections being of the mushroom unsupported area type with a somewhat nebulous safety limit. The source of the nitrogen was a commercial cylinder under ordinary commercial pressure, maximum about 2,000 psi. Two intensifiers with a suitable system of valves permitted charging the high pressure apparatus to an initial pressure of 2,000 kg/cm². Pressure above 2,000 was reached with a single stroke of the high pressure piston.

The germanium I owe, as before, to the courtesy and interest of Dr. William Shockley of the Bell Telephone Laboratories. Measurements are here presented in the 0° − 200° − 7,000 kg/cm² range for four specimens, three of them new, of n-type germanium of specific resistances at room temperature of 34.9, 11.3 (old), 6.6, and 0.004 ohm cm, and on three specimens, two of them new, of p-type, of specific resistances at room temperature of 2.1, 1.5 (old), and 0.00079. The n-type specimens will be referred to as Nos. 1, 2, 3, and 4 respectively, and the p-type as Nos. 5, 6, and 7. The range of intrinsic resistance in these specimens is considerably greater than before, so that we are here concerned with an essentially three parameter problem, that is, with the resistance of germanium as a function of pressure, temperature and degree of purity. In addition to measurements in the 7,000 kg/cm² range, measurements to 30,000 kg/cm² at room temperature were made on the three new n-type specimens and the two new p-type specimens, and in addition on a new n-type specimen of specific resistance 0.24 (No. 8) and two new p-type specimens of specific resistances 0.87 and 0.93 (Nos. 9 and 10). No attempt was made to repeat the former measurement to 100,000 kg/cm² on any of the new material, the internal evidence of the former measure-

ments having been that strain effects due to the non-hydrostatic medium are particularly large and difficult to interpret for germanium. I believe, however, that the strikingly anomalous effects above 30,000 must be kept in mind for any complete theory of the pressure effects.

The only new problem of technique which requires mention was connected with insulation at the higher temperatures. This problem was satisfactorily solved by the use of " Ceroc-T " wire from the Sprague Electric Co. (double teflon-ceramic coated) for the leads through the connecting pipe. In the lower cylinder proper insulation was secured with sheet teflon, which I owe to the courtesy of the DuPont Co., disposed at strategic situations. In all cases measurements were made by the potentiometer method with four leads. These were soldered into fine grooves cut transversely of the specimen. Pure tin was used for solder, the melting point of ordinary lead-tin solder being too low. The specimens were in the form of rods of square section, of the general order of dimensions of 2 cm long by 3 mm square, and were cut from single crystals. In the following, results are given in terms of specific resistance. These specific resistances were calculated from the measured resistances and the dimensions at room temperature and atmospheric pressure, disregarding changes of dimensions arising from temperature or pressure changes. Any corrections for such changes of dimensions are small and within errors of reproducibility of the material, there being measurable differences of specific resistance in specimens cut from the same original crystal.

THE RESULTS

A. Resistance to 200°C and 7,000 kg/cm².

The principal objective in making measurements at higher temperatures was to get the temperature into the intrinsic range, that is, the range where the primary contribution to the conductivity is provided by the germanium rather than the impurity. Entrance into the intrinsic range is roughly marked by reversal of the sign of the temperature coefficient of resistance, in the intrinsic range resistance decreasing with rising temperature, whereas at lower temperatures where conductivity is provided predominantly by
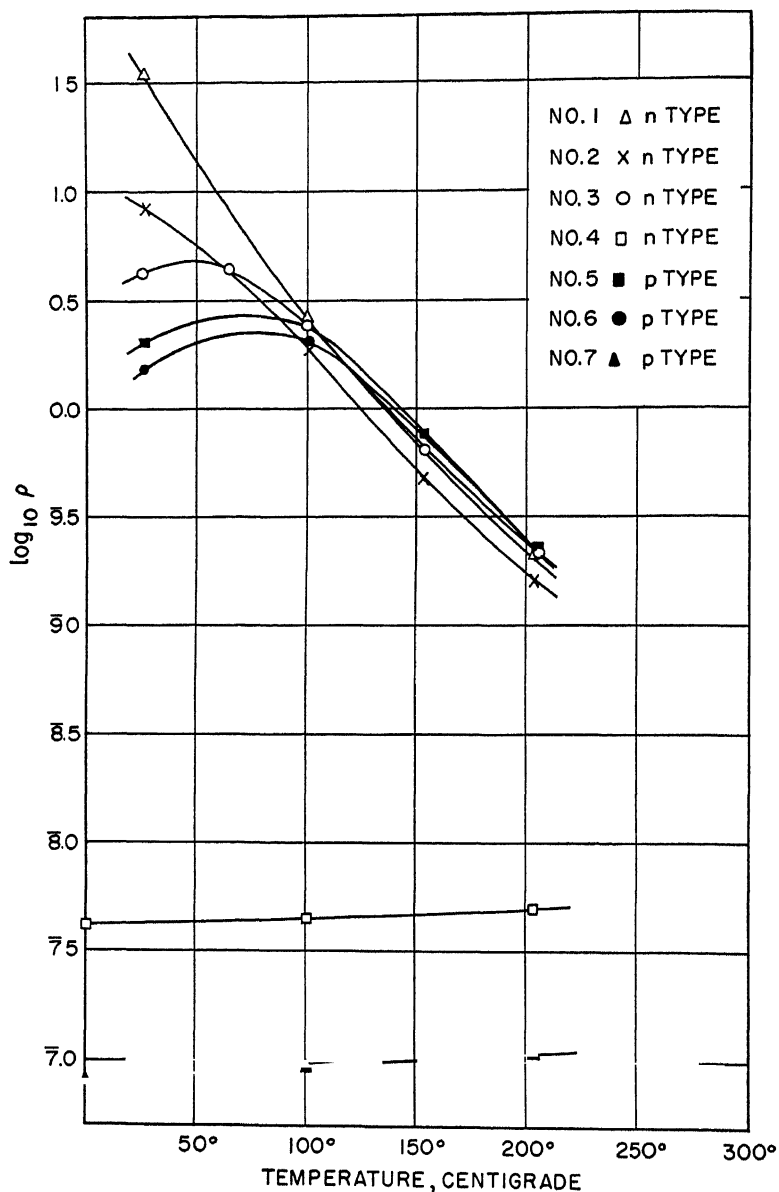
Figure 1. The specific resistance at atmospheric pressure of several specimens of germanium as a function of temperature. The specimens may be identified from the description in the text.

the impurity resistance increases with increasing temperature. The expectation was that in the intrinsic range the effect of pressure on resistance would be independent of the type of impurity that imparts the conductivity at lower temperatures, i.e., the same for n- and p-types. Of course this is to be expected only
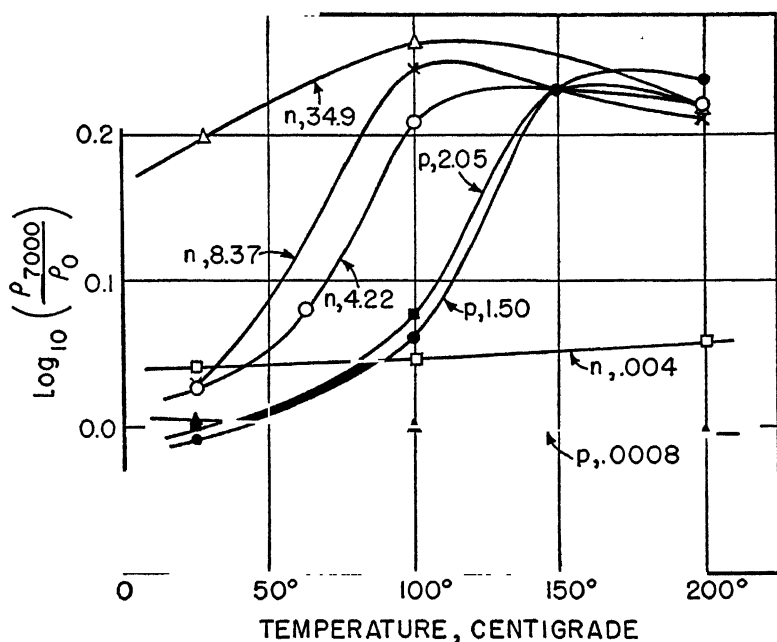
Figure 2. The logarithm of the ratio of resistance at 7,000 kg/cm² to that at atmospheric pressure as a function of temperature for various specimens of germanium. The labels on the individual curves indicate whether n- or p-type and the specific resistance at room temperature in ohm cm.

within reason, in a range where the impurity is not present in such excessive amount as to swamp the effect of the germanium itself. This expectation was gratifyingly fulfilled.

In Figure 1 is shown the logarithm of the specific resistance at atmospheric pressure of the various specimens as a function of temperature. The five specimens of which the specific resistance at room temperature varies from 1.6 to 34.9 roughly agree above

100°, that is, above 100° these specimens are in the intrinsic range. The two very heavily doped specimens of initial specific resistances 0.004 and 0.0008, however, are obviously of a different type, the conductivity of the impurities greatly preponderating over that of the germanium up to 200°. These specimens would be expected to become intrinsic, if at all, only at considerably higher temperatures.

With regard to behavior under pressure, the expectation is that the five upper specimens of Figure 1 will behave approximately the same above 100°, but that there is no reason why the other two, heavily doped, should exhibit similar behavior in our range. Figure 2 shows the behavior under pressure. The complete pressure behavior of the five specimens with specific resistance greater than 1 can be reproduced in this figure because the relation between pressure and logarithm of resistance is linear in pressure for all five, so that a single parameter suffices to reproduce the pressure behavior. The parameter thus exhibited in Figure 2 is $\log_{10}(R_{7000}/R_0)$. The pressure behavior of the two heavily doped specimens is, however, not linear in pressure, so that it cannot be reproduced by a single parameter. In order to permit a rough qualitative comparison, however, the same parameter is shown in Figure 2 for these two specimens also. Their behavior in detail under pressure will be described in the next paragraph. The figure shows in the first place that above 150° the effect of pressure on resistance of all five moderately doped specimens is approximately the same, as was expected. This effect is an increase of resistance under 7,000 by roughly $\log^{-1} 0.22$, or by 1/6, making the average pressure coefficient of resistance 2.4×10^{-5}. At room temperature the p-type specimen of resistance 1.5 has a small negative pressure coefficient, whereas the other p-type specimen of resistance 2.05 shows no measurable effect of pressure. The n-type specimens at room temperature show a smaller positive pressure coefficient than at higher temperature, the smaller the specific resistance (that is, the greater the amount of impurity) the smaller the coefficient. A short extrapolation indicates that the pressure coefficients of the 6.6 and 11.3 specimens will become negative at temperatures not far below 0°C. The pressure coefficient of the purest n-type specimen, of resistance 34.9, is not far from constant over the entire temperature range. All five specimens show a

maximum pressure coefficient between room temperature and 200°, the indications being, therefore, that at still higher temperatures the pressure coefficients will become progressively less. However, extrapolation to an eventual reversal of sign at much higher temperatures would obviously be hazardous.

The numerical results shown in Figure 2 are reproduced in Table I for the five moderately doped specimens, and in Tables II

TABLE I

EFFECT OF PRESSURE ON RESISTANCE IN 7,000 kg/cm² RANGE

Specimen		Temperature °C	$\log_{10}\rho_{7000}/\rho_0$
		n-type	
No. 1	$\rho_{27°} = 34.9$	27°	+0.197
		99.6°	+0.260
		199.9°	+0.213
No. 2	$\rho_{26°} = 8.37$	26°	+0.030
		100.0°	+0.242
		150.0°	+0.231
		200.0°	+0.214
No. 3	$\rho_{25°} = 4.22$	25.2°	+0.026
		64.3°	+0.080
		99.8°	+0.207
		149.9°	+0.228
		200.2°	+0.219
No. 4	$\rho_{0°} = 0.00410$	0°	+0.0412
		100.2°	+0.0474
		199.7°	+0.0600
		p-type	
No. 5	$\rho_{25°} = 2.05$	25.1°	.000
		100.0°	+0.078
		149.7°	+0.229
		200.6°	+0.218
No. 6	$\rho_{25°} = 1.505$	25°	−0.009
		100.0°	+0.060
		149.9°	+0.230
		200.2°	+0.237
No. 7	$\rho_{0°} = 0.000794$	0°	+0.00280
		100°	−0.00045
		200°	−0.00363

TABLE II

EFFECT OF PRESSURE ON RESISTANCE OF
No. 4 n-type GERMANIUM

Pressure kg/cm²	$0°$	$\log_{10}\rho$ $100.2°$	$199.7°$
0	7.61278	7.63949	7.69810
1,000	7.61753	7.64390	7.70448
2,000	7.62151	7.64962	7.71115
3,000	7.62801	7.65569	7.71888
4,000	7.63390	7.66266	7.72762
5,000	7.64025	7.67013	7.73704
6,000	7.64693	7.67827	7.74713
7,000	7.65390	7.68688	7.75804

TABLE III

EFFECT OF PRESSURE ON RESISTANCE OF
No. 7 p-type GERMANIUM

Pressure kg/cm²	$0°$	$\log_{10}\rho$ $100°$	$200°$
0	6.89980	6.97110	7.03930
1,000	6.90020	6.97059	7.03863
2,000	6.90060	6.97020	7.03802
3,000	6.90100	6.97002	7.03748
4,000	6.90140	6.97006	7.03697
5,000	6.90180	6.97019	7.03649
6,000	6.90220	6.97039	7.03606
7,000	6.90260	6.97067	7.03565

and III for the heavily doped non-linear specimens. It will be seen that the behavior of these two latter specimens, one of n- and the other of p-type, is quite different. One point of similarity, however, is the relatively small absolute magnitude of the pressure effect over the entire temperature range. The p-type specimen, of specific resistance 0.000794, increases linearly with pressure over the 7,000 range at 0°. At 100° it initially decreases in resistance, with a flat minimum near 3,000 kg/cm², and an eventual rise above 3,000 at a slower rate than the initial drop. At 200° the resistance decreases up to 7,000 approximately linearly, but with recognizable upward curvature, and with approximately the same absolute value of the coefficient as at 0°. The n-type specimen of

specific resistance 0.00410 at 0° increases in resistance at all three temperatures, in all cases approximately linearly, but in all cases with recognizable upward curvature. The curvature and the average value of the coefficient become larger at the higher temperatures.

B. Resistance to 30,000 kg/cm² at Room Temperature.

Room temperature, being below the intrinsic range for all except perhaps specimen No. 1, no similarity is to be expected between the behavior of n- and p-types, and the results will be presented separately. In Figure 3 is given in a single diagram the effect of pressure to 30,000 on the logarithm of the specific resistance of four n-type specimens. Over a range of specific resistance of four powers of ten the sign of the effect is the same, namely an increase of resistance with pressure. This increase, furthermore, is not linear, but increases with marked acceleration beyond 10,000 or 15,000 kg/cm². The final total increase under 30,000 increases consistently with the absolute value of specific resistance. The initial rate of increase in the linear range does not, however, show any consistent variation with absolute value of specific resistance.

The specimen of highest purity, No. 1, exhibits marked deviation from linearity between logarithm of specific resistance and pressure in the lower range below 15,000, the curve being concave down toward the pressure axis. At 15,000 there is a reversal of direction of curvature with an abruptness giving a definite cusp when the plot is made on a suitable scale. Although logarithm of specific resistance is not linear for this specimen below 15,000, the resistance itself is linear. In Figure 4 the experimental points are plotted on a different scale for this specimen. Two runs were made, at slightly different temperatures (21.9° and 22.3°), the second run with points more closely spaced up to 20,000, in order to exhibit more definitely the break in direction. The sharpness and definiteness of this break in direction near 15,000 becomes increasingly obscured in the other specimens with increasing degree of impurity.

In theoretical speculations as to the significance of the accelerated rise of resistance above 15,000 it is to be remembered that the rise does not increase indefinitely but as shown by the previous
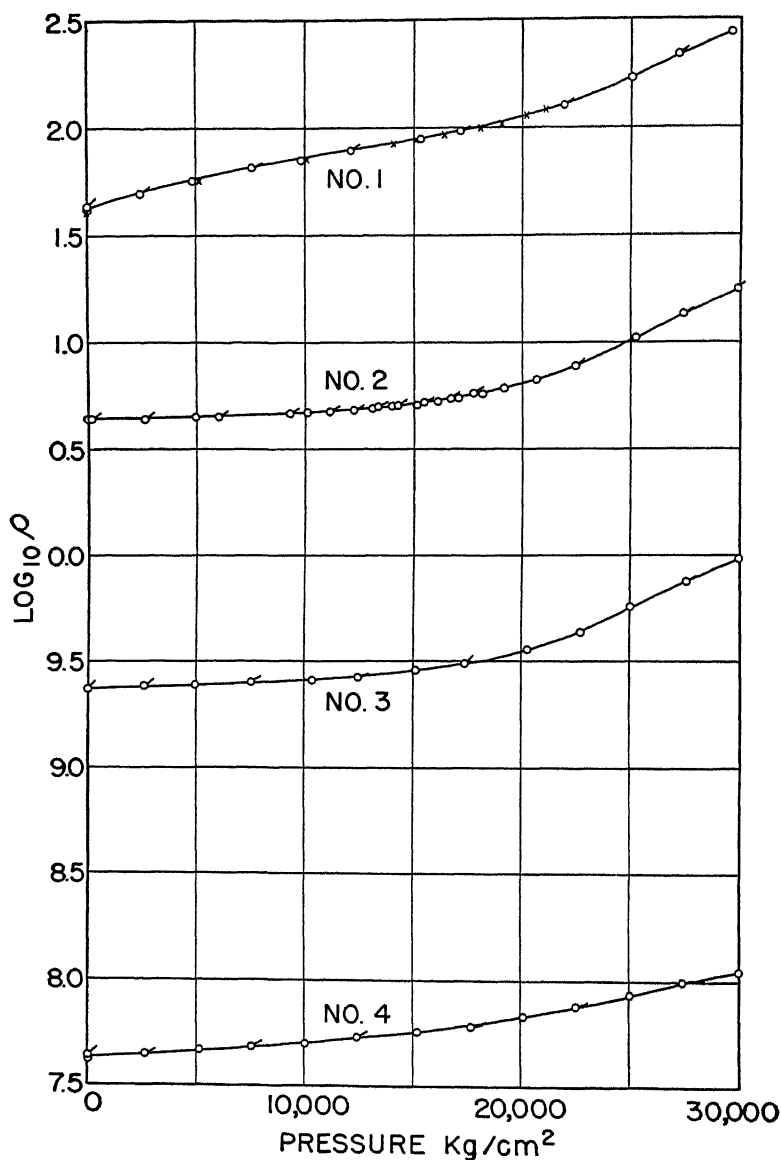
Figure 3. The logarithm of the specific resistance at room temperature of four specimens of n-type germanium as a function of pressure to 30,000 kg/cm².
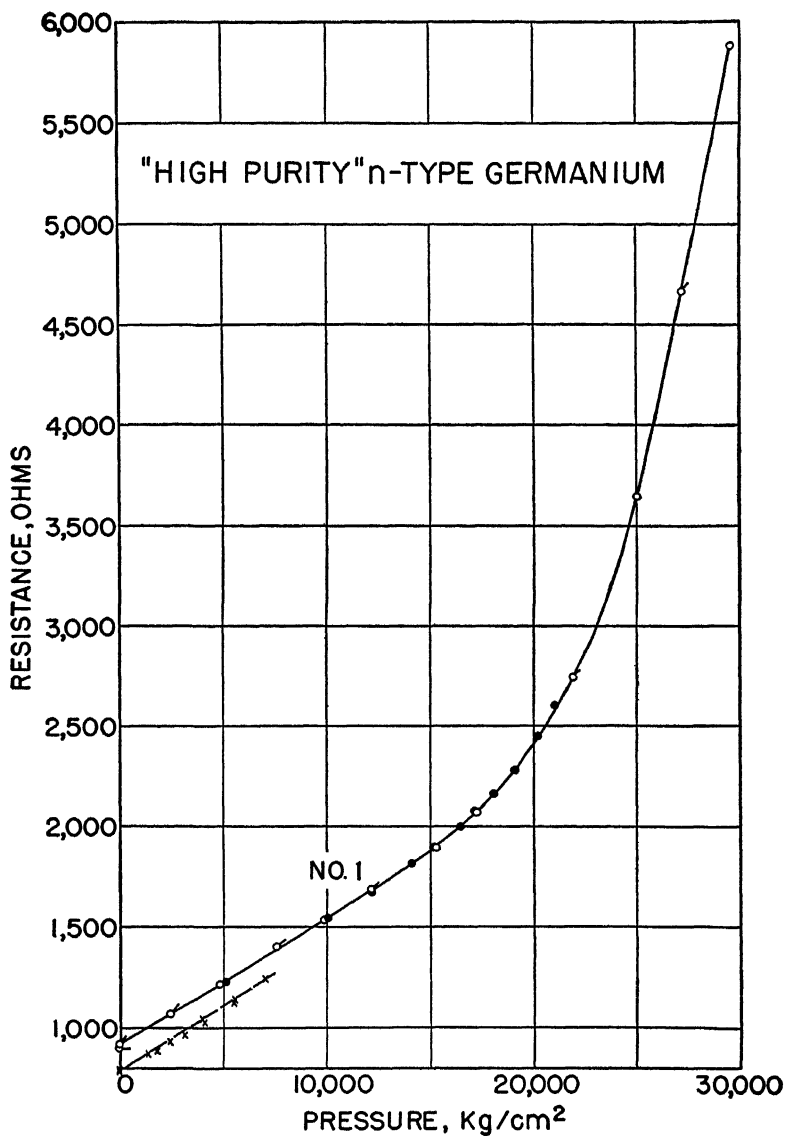
Figure 4. The actual resistance of the specimen of highest purity as a function of pressure to 30,000 kg/cm² (open and solid circles). The points indicated by crosses were obtained with the nitrogen apparatus of the first part of this paper. The initial slopes of the two sets of measurements are the same; the absolute value of resistance is not significant.

measurements to 100,000 is terminated by some sort of a catastrophe near 45,000, with rapid decrease of resistance beyond.

Previous measurements on the resistance to 30,000 at room temperature of p-type germanium of specific resistances 1.6 and 3.33 have given a nearly linear decrease of resistance over the entire range at the rate of −2.0 per cent per 10,000 kg/cm² for the first specimen and −2.2 per cent for the second specimen. In the present work similar measurements were made on two specimens of initial specific resistances at 24° of 0.866 and 0.932. These two specimens were cut from the same crystal; the variation of resistance is evidence of slight inhomogeneity in the material. Both of these specimens decreased in resistance under pressure at a rate approximately linear with pressure. The total decrease under 30,000 of the first specimen was 6.29 per cent and of the second 5.91 per cent, thus agreeing essentially with the previous results. To a second approximation the decrease is not linear with pressure, but decreases in rate at the higher pressures. Thus the average coefficient over the first 15,000 kg/cm² of the second specimen above was 2.16 per cent per 10,000 kg/cm² against an average coefficient over 30,000 of 1.97 per cent.

The heavily doped specimen of initial specific resistance 0.000794 was also measured. This *increases* in resistance linearly over the entire range within errors of measurement at a rate of 1.02 per cent per 10,000 kg/cm². The sign for the heavily doped p-type is thus the same as for the heavily doped n-type but numerically it is much less, being only one twentieth of the initial rate of the n-type.

I am much indebted to Mr. Charles Chase for assistance in making these experiments.

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    Harvard University, Cambridge, Mass.

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MISCELLANEOUS MEASUREMENTS OF THE EFFECT OF PRESSURE ON ELECTRICAL RESISTANCE

By P. W. Bridgman

CONTENTS

INTRODUCTION

In the following are collected a number of not very closely related measurements of the effect of pressure on the electrical resistance of metals, none of them extensive enough to justify publication separately, but all of them containing new information and presumably worth recording somewhere. There are in the first place measurements of the effect of pressure at higher temperatures than I have hitherto employed. For these measurements an old apparatus[1] designed for use at liquid oxygen temperature was restored and adapted to use up to 200°C, my previous limit having been 95°C. This restored apparatus was principally used for measurements on various specimens of germanium into the intrinsic range of temperature; these results are being published in a separate paper. Advantage is here taken of the availability of the apparatus to check to more than double the previous temperature range my former generalization that the pressure coefficient of resistance is approximately independent of temperature. This

check was made for only a few metals, choosing those for which it was anticipated that any effects might be largest. The pressure range of this work to 200° was only 7,000 kg/cm².

In the second place, opportunity was taken of the availability of highly purified specimens of tellurium, which I owe to the kindness of Professor Harry H. Hall, to explore more extensively than hitherto the resistance of this substance. Measurements are here presented both in the 7,000 range to 200° and in the 30,000 range at room temperature. These measurements have their bearing on the question of semi-conductors, which are at present attracting so much interest.

Finally, there are several metals available in greater purity than before or else not formerly available. Uranium in the form of wire of presumably greater purity than previously available was supplied by the Los Alamos Laboratory of the AEC. This was measured to 30,000 at room temperature. Hafnium is now available in this country; previous measurements to 12,000 are here extended to 30,000 and to 100,000 at room temperature. Metallic rhenium is available in the form of powder, but not in massive form. By shearing at 100,000 kg/cm² it is converted into a form sufficiently homogeneous to make it seem worth while to measure the effect of 100,000 kg/cm² on the electrical resistance. Specimens of rhenium sufficiently large for measurement with other sorts of apparatus were not produced by the shearing method. Finally, pure gadolinium in massive form was available from Dr. Spedding's laboratory of the AEC at Ames, Iowa, and measurements are here given at room temperature in the 30,000 and 100,000 ranges.

In addition to the measurements on electrical resistance, measurements were made incidentally on the shearing strength of a few of these materials up to mean pressures of 100,000 kg/cm², and also on the cubic compressibility of gadolinium.

THE RESULTS

In general comment on the measurements to 200°C and 7,000 kg/cm², the apparatus and method have been sufficiently described in the preceding paper on germanium.[2] Slight and obvious enough changes were demanded in the electric circuits in order to deal

with the lower resistance of the metals measured here as compared with germanium. Five metals were so measured: tin, lead, cadmium, aluminum and copper. It is probably to be expected that any enhancement of the previous very small effect of pressure on temperature coefficient of resistance (or of temperature on pressure coefficient) due to the present more extensive temperature range will be greatest on those metals with the lowest melting points, simply because the general loosening up of the structure by temperature is greatest for the low melting metals. The effect would therefore be expected to be greatest for tin and least for copper. I think it is also to be anticipated that the effect, such as it is, will be positive, that is, an arithmetic increase of pressure coefficient at higher temperatures.

Tin. This was " Kahlbaum " grade, from my stock of many years ago. It was extruded to wire 0.010 inch in diameter, and wound bare in the grooves of a pipestone spool. Measurements were made at three temperatures: 0°, 100.1° and 203.8°. The relation between pressure and resistance was very approximately linear, but with perceptible departure in the normal direction, that is, a decreasing coefficient at higher pressure. No attempt was made to evaluate the second degree term. The average pressure coefficients to 7,000 kg/cm² were -9.47×10^{-6}, -9.64×10^{-6}, and -10.13×10^{-6} respectively, at the three temperatures. This behavior is thus in line with that expected. Furthermore, the effect becomes accelerated at the higher temperatures, the extra hundred degrees of the present measurements magnifying the comparatively small effect in the former temperature range. The average temperature coefficient at atmospheric pressure was 0.00435 between 0° and 203.8° and 0.00440 between 0° and 100.1°. It is more usual for the temperature coefficient to increase with rising temperature.

It is to be mentioned that tin is non-cubic (tetragonal), and that strictly a single crystal should have been used rather than extruded wire. However, it is known that the departures from isotropy of tin are small, both with respect to resistance and compressibility, so that complications due to the extruded form are probably unimportant.

Lead. The material was of 99.9999 per cent purity, obtained a number of years ago from the Federated Metals Corporation, and used in many of my previous measurements. For the present

measurements it was extruded cold to wire of 0.010 inch diameter and wound non-inductively on a pipestone spool. Measurements were made at 0° and 204.0°. The average temperature coefficient of resistance at atmospheric pressure over this range was 0.00427. The relation between pressure and resistance departs notably from linearity in the normal direction, but no attempt was made to get the exact deviation from linearity. The average pressure coefficient to 7,000 kg/cm² was -1.286×10^{-5} at 0° and -1.360×10^{-5} at 204.0°. The change with temperature of the pressure coefficient is thus in the expected direction. Previous measurements[3] at 30° and 75° had given an increase of mean coefficient to 5,000 kg/cm² of 0.73 per cent for the temperature increment of 45°, against a present increment of 5.7 per cent for a temperature increment of 204°. The temperature effect on pressure coefficient thus increases more rapidly than linearly with increasing temperature, again in line with expectations. The increment 5.7 per cent is to be compared with an increment of 7.0 per cent for tin for the same temperature range. Tin, the lower melting metal, thus has the greater increment, again in line with expectations.

Cadmium. This was spectroscopically pure material from the New Jersey Zinc Company, extruded to wire of 0.010 inch diameter. Runs were made at 0° and 203.9°. At 0° the mean coefficient to 7,000 kg/cm² was -8.31×10^{-6}, with slight deviation from linearity in the normal direction. At 203.9° there was an upward jump in resistance of 0.6 per cent between 2,400 and 4,000 kg/cm². It seems probable that this jump is connected with the transition previously found[4] in cadmium at lower temperatures. This transition is somewhat obscure and has been found up to now only in single crystal material. The initial coefficient in the present measurements at 203.9° before the transition was -8.81×10^{-6}; after the transition the average coefficient to 7,000 was -9.81×10^{-6}. The transition did not run in the reverse direction on releasing pressure to atmospheric at 203.9°, neither did it run on lowering temperature again to 0° at atmospheric pressure, but there was a permanent increase of resistance at 0° of 2.4 per cent. The mean temperature coefficient of resistance between 0° and 203.9° at atmospheric pressure was 0.00462 before the transition and 0.00449 after it.

No clean-cut interpretation of the results is obvious. It is known

that the transition is such that the original crystal orientation is not recovered even when the reverse transition runs. The meaning of the results is further obscured by the fact that cadmium is not cubic and the differences in different directions in the crystal are important. The results before the transition do indicate, however, an effect of temperature on the pressure coefficient in the expected direction.

Aluminum. This was the identical specimen as previously measured[1] in the same apparatus at liquid oxygen temperature. Runs to 7,000 kg/cm^2 were made at 0° and 203.9°. The average temperature coefficient over this range at atmospheric pressure was 0.00460. The relation between pressure and resistance was linear within the error of measurement. At 0° the mean pressure coefficient to 7,000 was -4.26×10^{-6} and at 203.9° -3.90×10^{-6}. The temperature effect is thus not in the expected direction. The direction now found is, however, consistent with the variation previously found in the range below 0° down to $-182°$. Aluminum was unique among the fifteen metals previously measured in this range in the magnitude of the temperature effect.

Copper. Commercial copper wire was used, 0.003 inch in diameter, teflon covered. Runs were made at 0° and 204.2°. The mean temperature coefficient of resistance at atmospheric pressure in this temperature range was 0.00433. The relation between pressure and resistance was sensibly linear in the range to 7,000, and the identical mean coefficient was found for both temperatures, -1.89×10^{-6}. This is in line with expectations that the temperature effect is smaller for the higher melting metals.

Tellurium. Extensive measurements have been made on highly purified tellurium by H. H. Hall[5] up to 10,000 psi and at temperatures between 0° and 90°C. Professor Hall most kindly provided me with specimens of tellurium for further investigation. It was in the first place of interest to find whether there is an important change of behavior over my somewhat wider range with the nitrogen apparatus, that is, up to 7,000 kg/cm^2 (100,000 psi) and 200° C. Measurements were made on one specimen in the nitrogen apparatus. The specific resistance, ρ, of this specimen at atmospheric pressure at dry ice temperature ($-78°$) was 3.73 ohm cm. This corresponds to one atom of acceptor impurity per 10^7 atoms of tellurium. The measurements disclosed nothing novel, and the

numerical results were in line with what was to be expected from the measurements of Hall. Logarithm of resistance against pressure is concave upward (diminishing slope at higher pressures) and the average slope is less at the higher temperature. At $0°$ $\log_{10}\rho_0/\rho_{7000}$ $= 0.958$. The value which $\log_{10}\rho_0/\rho_{7000}$ would have had if the relation had been maintained linear over the entire pressure range was 1.201. This corresponds to an initial rate of decrease of resistance of 2.82 per cent for $1,000$ psi, which is only slightly higher than the value of Hall for a specimen of not quite as high purity. At $203.9°$ the corresponding values for $\log_{10}\rho_0/\rho_{7000}$ were 0.796 and 0.910. The latter is equivalent to an initial rate of decrease of 2.13 per cent per $1,000$ psi.

Advantage was taken of the possibility of obtaining material of known and high purity to examine further the effects of considerably higher pressures at room temperature. In particular, it was desired to explain the much smaller effect of pressure found in my most recent measurements to $100,000$ as compared with my previous measurements to $30,000$. Two possible explanations offered themselves. One was that the shearing strains incident to the transmission of pressure by $AgCl$ in the measurements to $100,000$ produce large changes of resistance. A second possibility, suggested by Professor Hall, was that during the extrusion in preparing the material for measurement to $100,000$ sufficient impurity, presumably iron, had been introduced to importantly depress the pressure coefficient, it having been Professor Hall's experience that comparatively small amounts of impurity do this. To settle this question it was proposed to make measurements to $30,000$ on the highly purified material of Professor Hall, then to extrude these through steel dies and recast them to the original shape and redetermine the effect of pressure.

Measurements were carried out on three different specimens. I first measured the resistance at room temperature to $30,000$ kg/cm². I then melted each specimen in high vacuum in a pyrex tube of ¼ inch diameter. It was solidified in the tube, removed to a steel die, and extruded to ⅛ inch diameter at approximately $350°C$. This extrusion left the specimens very rough, with many fissures. Professor Hall then " melted in helium in pyrex which had been cleaned in hot chromic acid cleaning solution and rinsed in distilled water until the electrical conductivity of the wash

water was less than one part per million referred to NaCl." After melting, two of the specimens were " quenched " by removing from the hot zone of the furnace into a region at room temperature. The third was recrystallized by raising the furnace heater slowly

TABLE I

SUMMARY OF RESULTS FOR TELLURIUM

	Specific Resistance at $-78°$		$R_0/R_{30,000}$		
Specimen	Original	After Extrusion and Melting	Original	After Extrusion	Remarks
1	1.7 ohm cm	0.59	195 360	537	quenched
2	2.5	0.038	209 347	39 76	quenched
3	4.2	0.41	145 282	288	slowly cooled
single crystal	0.5		400 600		two orientations

so that the sample was progressively cooled, starting at one end, in about an hour. This latter treatment has been found to be especially effective in removing impurity. The three specimens thus prepared were then remeasured by me to 30,000 at room temperature. The results are given in Table I. It will be seen that two figures are given in several instances for the ratio R_0/R_{30000}. There are two reasons for the uncertainty thus indicated. On release of pressure there may be a permanent increase of resistance. The two figures (39 and 76) given for specimen No. 2 after treatment are the values obtained using the initial and the final values of R_0. In addition, during the first series of measurements, an uncertainty was introduced by a loose electrical connection which was not discovered until the termination of those measurements. The two figures given in the column of original measurements include both any effect from a possible permanent change of zero and the maximum and minimum possible effects of the loose connection.

It will be seen from Table I that the permanent decrease of resistance after extrusion and presumable introduction of iron as

an impurity is much larger for specimen No. 2 than for the two others. It is also for specimen No. 2 that the decrease of pressure effect is large and unmistakable, as suggested by Professor Hall. For the other two specimens, however, for which the diminution of resistance was less, the pressure effect is actually increased. It would appear therefore that in the range of comparatively slight impurity there are other effects on the pressure coefficient more important than impurity. These effects are, furthermore, capricious, as emphasized by a comparison of the data for my former single crystal material,[7] included in the table, with the results for the present material. It appears probable that two related factors are primarily responsible for this capriciousness, both connected with the failure of the specimens to be single crystals. In the first place, the resistance is different in different directions in the crystal, being greater by 75 per cent in one direction than another, with an 8 per cent variation of pressure coefficient in these directions. This means that specific resistance by itself is not an adequate parameter to completely describe the nature of the material. In the second place, the cubic compressibility varies with direction in the crystal by an abnormally large amount for tellurium, there being differences of sign in different directions. This means that when a multi-crystalline specimen is exposed to hydrostatic pressure shearing stresses are developed, which may conceivably lead to fracture for high enough pressures, particularly in this unusually brittle material. This is consistent with my observation that always after exposure to 30,000 kg/cm^2 a number of cracks appeared in Professor Hall's specimens, although there were none in my single crystal material. Professor Hall's pressure range was not sufficient to result in cracking in his own experiments.

Several conclusions may be tentatively drawn. My unexpectedly small pressure effect in the experiments to 100,000 kg/cm^2 may well be a combination of effects of impurity introduced by the extrusion process and the shearing stresses incident to non-hydrostatic stress. For tellurium of purity greater than say 0.5 ohm cm at $-78°$ the effect of impurity on pressure coefficient is probably less important than that of non-isotropy and failure of unicrystallinity. For reproducible and completely significant results on high purity tellurium it would appear essential that the material be unicrystalline and that the crystal orientation be known.

Uranium. This I owe to the courtesy of the Brookhaven National Laboratory. The point in repeating former measurements[8] to 12,000 kg/cm² was that this new material would presumably be purer. It was supplied in the form of " wire . . . 6 inches long and 0.020 inch in diameter, weighing 0.648 gram, of normal uranium isotopic constituency. It is approximately 99.9 per cent pure, and can be handled as unclassified material." Four leads were attached by spot welding, and measurements made to 30,000 kg/cm² in my regular way. Results were normal in every respect; the numerical values are given in Table II.

TABLE II

EFFECT OF PRESSURE ON RESISTANCE OF URANIUM

Pressure kg/cm²	$-\dfrac{1}{p}\dfrac{\Delta R}{R_0}$
0	4.880×10^{-6}
5,000	4.71
10,000	4.56
15,000	4.436
20,000	4.317
25,000	4.206
30,000	4.104

Former measurements[8] had given a linear drop of resistance with pressure over the range to 12,000 kg/cm² with a mean coefficient -4.36×10^{-6}. The pressure effect on the present specimen is thus somewhat larger than on the former; the difference is in the direction to be expected from greater purity in the present specimen. The mean temperature coefficient of resistance at atmospheric pressure between 0° and 100° of the present specimen is 0.00248 against a former value 0.00230. The difference is not large, but again in the direction of greater purity for the present specimen. It would appear probable that the temperature coefficient of this metal in the pure state is significantly lower than the traditional value for a pure metal, $1/T$ or 0.00367.

In the normal course of events I would also have measured the resistance of this new material with my new technique[6] to 100,000 kg/cm². This, however, would have involved mutilation of the

specimen, and since this would have involved considerable red tape to obtain the necessary permission and presumably would have given results not greatly different from those formerly found, I chose the easiest way, and returned the specimen as received to Brookhaven, as requested.

Rhenium. This was obtained in the form of powder from Eimer and Amend via the Brookhaven National Laboratory. Apparently there are no measurements whatever on this material in massive metallic form. An exploratory examination for possible new polymorphic forms at high pressure was made by the shearing technique[9] with a new carboloy apparatus to 100,000 kg/cm². The shearing measurements were perfectly smooth over the entire range, indicating no polymorphic transitions. The general magnitude of the plastic flow strength was approximately 20 per cent greater than that of copper, and the manner of variation with pressure was about the same as for copper. The product of the shearing was a coherent disc of metallic appearance of sufficient mechanical strength to invite a determination of its properties, in view of our complete ignorance of any such. The only property readily measurable on such small specimens was the electrical resistance to 100,000 kg/cm², for which the dimensions were well adapted. Runs were accordingly made by the regular technique[6] on two specimens to 100,000. Below 20,000 the two runs differed considerably, as is usual, but above 20,000 the agreement of the two runs was rather good, one giving the value 0.712 for the ratio $R_{100,000}/R_{20,000}$ and the other 0.701. The average for the two runs is given in Table III. The pressure effect on resistance is of the general order of that characteristic of the softer metals, and is not far from that of tin.

A very rough value for the specific resistance at atmospheric pressure at room temperature is 6×10^{-4} ohm cm; doubtless this value is too high. The most secure of the present results with this new material would seem to be that there is no discontinuity up to 100,000 kg/cm² at room temperature, a result given by both shearing and resistance measurements.

Hafnium. The electrical resistance and compressibility of hafnium have been previously measured[10] up to 12,000 kg/cm² in 1928. The material was loaned by the Research Laboratory of the Philips Lamp Works in Eindhoven, Holland, and was returned to them

after the measurements. Zirconium, which is similar chemically to hafnium, has recently been found [11] to have a transition in the neighborhood of 80,000 kg/cm². It therefore appeared of interest to extend the measurements on hafnium to higher pressures. Some difficulty was experienced in locating suitable material. The Eindhoven Laboratory had lost theirs during the German occupation. On running down a number of references in the recent literature to properties of hafnium no suitable material was uncovered.

TABLE III

EFFECT OF PRESSURE ON RESISTANCE OF RHENIUM

Pressure kg/cm²	$R_p/R_{20,000}$
10,000	1.140
20,000	1.000
40,000	.856
60,000	.787
80,000	.742
100,000	.706

Finally, through the Foote Mineral Company, I was put in contact with work by the U. S. Air Force on this material, who were interested in it from the point of view of jet engines. A small quantity of pure massive material was kindly made available for my measurements by the Wright Air Development Center at Dayton, Ohio. Various physical properties of this material have been described in print.[12] Of the several specimens of hafnium studied in that paper only one, bar No. 778, " as deposited iodide crystal bar " had high presumptive purity, as indicated by the agreement of its physical properties with previously published values. This purity was stated, without analysis, to be indefinitely better than 99 per cent. My specimen was a small block cut with a saw from bar 778. For my measurements two small slivers were cut from the block and rolled in four passes to a thickness of 0.007 inch from an initial thickness of the order of 0.040 inch. One of these pieces was rolled without annealing, whereas the other was annealed at every pass in quartz in vacuum at a temperature of approximately 1050°C. The rolled strips were finally reduced by hand filing to a thickness of 0.002 inch.

Two sets of measurements were made: of resistance at room temperature to 30,000 kg/cm² with true liquid as the transmitting medium, and of resistance at room temperature to 100,000 with AgCl transmitting. For the 30,000 measurements dumbbell shaped pieces were cut from the rolled strip, the isthmus of the dumbbell being 0.9 cm long and 0.05 cm wide, and two terminals were attached by spot welding to each end of the dumbbell. Measurements at 30,000 gave smooth regular variation of resistance. Resistance decreases under pressure, with normal curvature. The coefficient is small compared with that of most metals. Within the limit of error, which was not anywhere greater than one part in 300 on the total change of resistance, decrease of resistance can be represented by a second degree formula in the pressure. This means that $\dfrac{1}{p}\dfrac{\Delta R}{R_0}$ is linear in pressure. For the unannealed specimen the value of $\dfrac{1}{p}\dfrac{\Delta R}{R_0}$ ran from -8.78×10^{-7} at atmospheric pressure to -7.06×10^{-7} at 30,000 kg/cm². For the annealed specimen the corresponding values were -9.31×10^{-7} and -7.54×10^{-7}. The latter is to be preferred as more truly representative of the metal. The mean temperature coefficient of resistance at atmospheric pressure between 0° and 100° was 0.00419, and the specific resistance at 0° 29.7 $\times 10^{-6}$ ohm cm. The latter figure is only rough because of uncertainty in estimating the correct effective length of the specimen; it is to be compared with the value 32.0 $\times 10^{-6}$ for the same material by Adenstedt. My previous measurements [10] to 12,000 kg/cm² on hafnium had given at 30° a linear relation between pressure and resistance with a mean coefficient of 10.0 $\times 10^{-7}$. The former specific resistance at 0° was 31.9 $\times 10^{-6}$ and the mean temperature coefficient between 0° and 100° 0.00398.

Measurements of resistance to 100,000 were made by the regular technique on two specimens from the same annealed strip as used to 30,000. Both runs to 100,000 gave smooth results, with no suggestion of any transition, such as had been shown by zirconium. In terms of the resistance at 20,000 as fiducial the changes in proportional resistance at 100,000 given by the two runs were 0.0338 and 0.0322, which is rather good agreement for material with as small an absolute effect as this. The two runs were averaged, and

the decrement of proportional resistance reduced uniformly by
21 per cent over the entire range in order to secure agreement
with the previous measurements in the range to 30,000. The final
results for the proportional change of resistance are given in Table
IV. The readings below 10,000 were taken from the 30,000 runs.
Gadolinium. The material was in the form of a massive hunk
weighing about 3 grams prepared by the AEC at Ames, Iowa,
under the direction of Dr. Frank Spedding. This is the first time

TABLE IV

EFFECT OF PRESSURE ON RESISTANCE OF HAFNIUM

Pressure kg/cm²	$-\dfrac{\Delta R}{R_0}$
0	.0000
10,000	.0087
20,000	.0163
30,000	.0227
40,000	.0279
50,000	.0316
60,000	.0348
70,000	.0374
80,000	.0393
90,000	.0408
100,000	.0419

that this material has been available for my measurements. Four
different sorts of pressure measurement were made: shearing to
100,000 kg/cm², electrical resistance to 100,000, electrical resist-
ance to 30,000, and cubic compressibility to 40,000. Four metals
in the rare earth group have previously been measured [6] to high
pressures: cerium, lanthanum, praseodymium, and neodymium.
All have shown anomalies, but no two alike. The question of
major interest here was whether gadolinium would also be anom-
alous, and if so, what is the nature of the anomaly. The results
may be anticipated to say that an anomaly was found, different
from any of the others.

The measurements of resistance to 30,000 at room temperature
have the greatest relative accuracy, and they will be described
first. The specimens were prepared in much the same way as
hafnium. A small sliver was cut from the original hunk with a

cold chisel, rolled in four stages from 0.025 to 0.004 inch thick, with annealing in quartz at high vacuum to approximately 1050°C after each rolling. From the strip thus obtained a dumbbell shaped piece was cut and two terminals of fine platinum attached by spot welding to each of the two broad tabs of the dumbbell. It was found incidentally that platinum rapidly alloys with gadolinium at high temperatures. Unlike the other four rare earth metals, gadolinium appears to be stable in air at room temperature. It was shipped to me freely exposed to air, and during my preliminary manipulations no precautions were taken to protect it from the air, as seemed unnecessary because of the appearance. Contrasted with its inertness in air is violent chemical activity in contact with " Eliot's " soldering fluid, a neutral flux often used where freedom from subsequent corrosion of the soldered connection is deemed essential. In contact with this fluid gadolinium bubbles violently and rapidly dissolves. The nature of the chemical attack was not investigated further; it is known that Eliot's fluid contains a large proportion of alcohol.

When exposed to 30,000 gadolinium exhibits an instability in resistance beginning at approximately 20,000. Below this, resistance is steady under constant pressure. Above 20,000 resistance at constant pressure drifts rapidly downward. This drift occurs in the entire region between 20,000 and 25,000. If there is any tendency to asymptotic cessation of drift at any pressure in this range it was too slow to be found in the course of ordinary manipulation. Between 25,000 and 30,000 readings are approximately constant at fixed pressure, but the slope of the curve of resistance against pressure is much greater than at lower pressures. On release of pressure reversal of drift does not occur, but readings are constant all the way back to atmospheric pressure, where the initial resistance is diminished by approximately 7 per cent. At atmospheric pressure there appears to be a very slow drift of resistance back toward the initial value; on standing three days at atmospheric pressure there was a one per cent recovery of resistance. Repetition of the pressure run now gave approximately the same curve as on the first release of pressure, but with hysteresis in the one per cent range corresponding to an internal change only one seventh of that at first. The virgin behavior of the material may be completely restored by an annealing after the pressure run

to 1050°C. How much lower a temperature would have sufficed was not investigated.

The averages of the two runs on freshly annealed material are given in Table V. Up to 20,000 the two runs agreed within one per cent on the change. The agreement was not so good during the transition and on release of pressure, the relative change of resistance for one run being 5 per cent greater than for the other (0.2049 against 0.1935). The permanent changes of zero after release of pressure were 0.0680 and 0.0767. It will be noticed that

TABLE V

Effect of Pressure to 30,000 kg/cm² on Resistance of Gadolinium

Pressure	$-\Delta R/R_0$	
kg/cm²	Increasing Pressure	Decreasing Pressure
0	.0000	.0723
10,000	.0459	.1317
20,000	.0885	.1709
25,000	.1711	.1862
30,000	.1992	

the departure from linearity is much greater on release of pressure, after the transition has run, than on the first application of pressure.

The temperature coefficient of resistance at atmospheric pressure between 0° and room temperature of the freshly annealed material was 0.00176, and of the material previously exposed to 30,000 0.000908 (identical values for the two specimens). Again a temperature coefficient is obtained for a presumably pure rare earth metal materially less than the traditional $1/T$.

The specific resistance at 0° of the annealed material was 140.5×10^{-6} ohm cm, and of the pressure exposed material 134.5×10^{-6}.

Measurements of resistance to 100,000 kg/cm² were made on two specimens cut from the same strips as had supplied the specimens for 30,000. Both of these runs showed the same effect between 20,000 and 25,000 as had the 30,000 series. This disturbance makes it impossible to estimate the reduction factor that would secure agreement between the 30,000 and the 100,000

series, so that the present series has had to be accepted at its face value between 30,000 and 100,000. The decrements of proportional resistance at 100,000, in terms of the resistance at 30,000 as unity, were 0.1904 and 0.2100 for the two runs. These were simply averaged for the final results, which are given in Table VI. In this table the values to 30,000 are copied from Table V. It will

TABLE VI

Effect of Pressure to 100,000 kg/cm² on Resistance of Gadolinium

Pressure kg/cm²	R/R_o
0	1.0000
10,000	.9541
20,000	.9115
30,000	.8008
40,000	.7637
50,000	.7322
60,000	.7056
70,000	.6844
80,000	.6668
90,000	.6523
100,000	.6405

be seen that the resistance above 30,000 decreases smoothly with pressure; there is no evidence of any further transition. This was confirmed by the shearing measurements to 100,000, which will be described in fuller detail in another paper. The transition at 25,000, or whatever it is, is reflected in the shearing measurements by a point of inflection with reversal of curvature from concave down to concave up. This abnormal upward concavity continues over the entire range to 100,000, but there is no new incident to indicate any further internal change. The absolute magnitude of the shearing strength to 100,000 is of the order of half that of copper.

Since the compressibility of gadolinium appears to have never been measured, opportunity was taken to measure it to 40,000 in my conventional apparatus,[13] and the results are given here for the sake of completeness, although not related to electrical resistance. Since gadolinium is somewhat stiff mechanically, that

method was employed in which the gadolinium is surrounded by a sheath of indium in order to transmit pressure uniformly. The compressibility of the indium was eliminated by making a new blank run with iron, the immediate results of the measurements being the differential compressibility between gadolinium and iron. Two set-ups were made on two different specimens; the volume decrement at 40,000 of the two sets differed by 0.37 per cent of themselves. Two complete runs were made with each set-up, embracing two excursions with increasing pressure and two with decreasing. Of these four excursions the three last gave the compressions of the material as modified by the quasi-permanent transition between 20,000 and 25,000 kg/cm². The two initial excursions gave a distinct break in the volume between 20,000 and 25,000 corresponding to the break in resistance. This break, which of course was in the direction of a decrease of volume, had the numerical magnitude $\Delta V/V_0 = -0.00432$. There was no perceptible break in the direction of the tangent on the two sides of the break; that is, the transition runs with no measurable change in compressibility. The final values are given in Table VII.

TABLE VII

Compression of Gadolinium to 40,000 kg/cm²

Pressure kg/cm²	$\Delta V/V_0$
0	.0000
5,000	.0125
10,000	.0242
15,000	.0351
20,000	.0452
25,000	.0545
30,000	.0632
35,000	.0712
40,000	.0785

These values are for the material as altered by a preliminary application of 25,000 or more. In order to obtain the volume decrements of the freshly annealed material it is sufficient to add to the above values 0.0043 from 25,000 on (inclusive).

The compression of gadolinium is distinctly less than that of the four other rare earth metals previously measured.[14] It ap-

proaches most closely to neodymium, for which the volume decrement under 40,000 is 0.0955 as compared with 0.0785 above for gadolinium.

In the course of the compressibility measurements the density at room temperature was incidentally determined. This was 7.964 at 26° for material previously exposed to pressure.

It will be interesting to measure the effects of high pressure on other members of the rare earth series of metals as soon as they become available. The rare earth metals as a class are characterized by incomplete inner electron shells. Under high pressure these shells may be expected to become occupied by electron transfer from outer shells, so that we have here perhaps the most favorable opportunity of studying pressure induced electronic rearrangement within the atom, a phenomenon which must become increasingly important for all atoms at higher pressures.

It is a pleasure to acknowledge the assistance of Mr. Charles Chase with these measurements.

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THE EFFECT OF PRESSURE ON SEVERAL PROPERTIES OF THE ALLOYS OF BISMUTH-TIN AND OF BISMUTH-CADMIUM

By P. W. BRIDGMAN

Contents

INTRODUCTION

Comparatively little is known of the effect of pressure on the properties of alloys. The results presented here are the beginning of what is hoped will become a systematic program of investigation. Various considerations present themselves in choosing the alloys for first investigation. One might, for example, expect interesting results with those alloys for which one or the other of the two pure components is already known to have interesting pressure effects. From this point of view bismuth, with its known abnormal properties under pressure and its various polymorphic transitions, is perhaps the most natural first candidate for one of the components. An additional consideration is the ease of handling of many of the alloys of bismuth – the melting point is low, chemical activity in the air is not serious, and most of the alloys are not so brittle as to make it difficult to get them into suitable geometrical shape for measurement.

In the following, results are presented for the two series, bismuth-tin and bismuth-cadmium. Bismuth-tin is known to form a simple eutectic series at atmospheric pressure.[1] At the two ends of the composition range solid solutions are formed up to definite composition limits; at intermediate compositions the system breaks up into a mechanical mixture of the two limiting compositions. Bismuth-cadmium, on the other hand, is not known to exhibit mutual solubility at all,[2] and at all compositions the system breaks into a mechanical mixture of the pure components. It is to be expected therefore that the pressure effects will be simpler on the bismuth-cadmium than on the bismuth-tin series. This proved to be the case. The results for the bismuth-tin series were further complicated by the unexpected entry of an intermetallic compound of bismuth and tin requiring 20,000 kg/cm² or more for its formation. As a consequence of this complication by far the larger amount of space in this paper is devoted to the bismuth-tin series.

The measurements presented in the following were all made by standard techniques which I have applied in the past to various pure metals.[3] The measurements include: electrical resistance to 30,000 kg/cm², pressure transmitted by a true liquid; electrical resistance to 100,000 kg/cm², pressure transmitted by AgCl; cubic compressibility to 40,000 kg/cm²; and shearing strength to

100,000 kg/cm². In addition, the following were measured at atmospheric pressure: density, specific resistance, and temperature coefficient of resistance.

The Bismuth-Tin Series

It will probably increase the perspicuity of the presentation to first briefly describe the nature of the novel effects found under pressure. Apparently the intermetallic compound BiSn becomes possible under pressure. This compound itself undergoes a polymorphic transition at 20,000 kg/cm², and it is only the high pressure modification of the compound which can be formed by direct transformation out of the previously known eutectic mixtures stable at atmospheric pressure. This means that on first increasing pressure on the virgin eutectic system nothing unusual happens until 20,000 or more is reached, when there is an irreversible transformation to the high pressure modification of the compound. The system after the transformation consists of the high pressure modification of the compound mixed in various proportions with the limiting solid solutions, depending on the gross composition. Further increase of pressure encounters the polymorphic transitions of bismuth, which occur in the general neighborhood of 25,000 and 27,000 kg/cm² for pure bismuth and which occur at very nearly the same pressures for dilute solutions of tin in bismuth. On release of pressure the transitions in which bismuth plays the dominating role are first encountered, and then, at a pressure in the neighborhood of 15,000 kg/cm², the reversible transition of the compound to its low pressure form. Release of pressure to atmospheric now gives a system composed partly of the original constituents and partly of the low pressure modification of the compound. A second increase of pressure first encounters the reversible transition of the compound near 15,000 with the other polymorphic transitions at higher pressures. A second release of pressure approximately reproduces the behavior on the first release. The low pressure modification of the compound is not indefinitely stable at atmospheric pressure at room temperature, but slowly reverts. The speed of reversion is much increased at elevated temperatures and may be practically suppressed at dry ice temperature. In the following a few measurements have been made of the speed of reversion.

The alloys were prepared by melting together in pyrex in vacuum weighed amounts of the pure components. The tin was old Kahlbaum " K " stock, and the bismuth was from my stock of specially purified material, purified first electrolytically and then by crystallization into single crystal form. The alloys were prepared in batches of approximately two grams. The composition was not checked after formation, but a check on loss of metal during the alloying was obtained by weighing the nugget after formation. Loss of material of as much as 0.001 gram was not common. The molten metals were violently agitated together and rapidly solidified in order to give a homogeneous product. The resulting nugget, after breaking away the glass and weighing, was scraped bright and divided into two approximately one gram lots. One of these was extruded to a wire of approximately 0.020 inch diameter for the resistance measurements to 30,000. For extrusion, temperature was raised to slightly less than the eutectic temperature, 140°. The extruded wire was of the order of one meter in length. Homogeneity of composition over the wire was checked in several instances by resistance measurements on specimens cut from the two ends. No examples of measurable inhomogeneity were found. The second one gram piece was used for the compressibility measurements to 40,000. Density determinations were made on this piece, usually both before and after exposure to pressure. Density was determined by weighing in air and in CCl_4. This fluid proved to be very much superior to water for this purpose; it is 50 per cent denser than water, the surface tension is low, it dissolves any surface film of grease on the specimen, and air bubbles do not cling to the specimen. Some seventeen different compositions were prepared for the measurements. In addition, there were five compositions left from the work of Ufford[4] in this laboratory in 1928. Ufford studied the resistance to 12,000 kg/cm², not high enough to encounter any of the new effects found here. *Density.* The experimental values are given in Table I and shown graphically in Figure 1. Densities are given both for the virgin specimen and after exposure to 40,000 kg/cm². The latter densities were measured within half an hour after release of pressure at room temperature, so that any error due to reversion of the compound should be small. It will be seen that at both ends of the composition range small increases of density are produced by

pressure, whereas in the middle of the range the effect is a much larger decrease of density. The relation between composition and density of the virgin alloy can be represented by two linear ranges joining at 39 At per cent with an error not anywhere greater than 0.02 on the density, except for the single point at 99 per cent Bi, where the deviation is 0.03. The densities at intermediate compositions are greater than would be given by a single straight line joining the two pure components.

TABLE I

DENSITY AT ATMOSPHERIC PRESSURE AT ROOM TEMPERATURE
BI-SN SERIES

Composition, At % Bi	Density Before Pressure	After Pressure	Change After Pressure
100.00	9.788		
99.05	9.745	9.784	+.039
97.98	9.759	9.763(?)	+.004(?)
89.88	9.588	9.612	+.024
79.89	9.391	9.350	−.041
70.05	9.160	9.131	−.029
63.27	9.014	8.879	−.135
49.82	8.734	8.614	−.120
43.4	8.578	8.568	−.010
36.2	8.418	8.418	.000
29.72	8.212	8.222	+.010
19.99	7.906	7.911	+.005
10.08	7.608	7.626	+.018
5.07	7.427	7.470	+.043
2.05	7.340	7.365	+.025
0	7.278		

The densities of this series have been previously determined by Thomas and Evans.[5] Their values agree on the whole with mine within 0.02 or usually better, but with two perhaps important exceptions. At 12.4 and 25 At per cent their densities are 0.05 higher than my curve, and between 98 and 100 per cent Bi three compositions indicate a sharp maximum at 99 per cent at a density 0.08 greater than the density of pure Bi. I have only a single point in the close neighborhood of 99 per cent, but for this there is no rise but rather a drop of about 0.03 on the density below what would be indicated by a straight line. Preparation of other specimens would be desirable to straighten out the discrepancy.
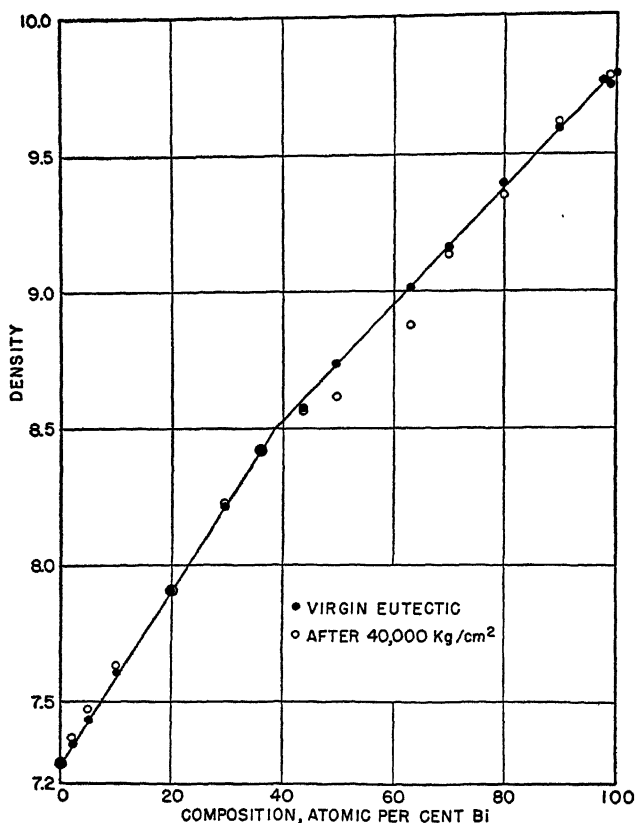
Figure 1. Densities of bismuth-tin alloys at atmospheric pressure at room temperature, before and after exposure to pressure, as a function of composition.

This I did not do because the principal interest of this work is in other ranges of composition.

Compressions to 40,000 kg/cm². Measurements were made with the same apparatus and with the same technique as my most recently published values on this same topic. All the alloys of the present series were soft enough mechanically so that the use of an indium sheath to equalize pressure was not necessary. Because of the permanent changes produced by the first application of pressure, two excursions to the maximum pressure were regularly

made on all specimens except the pure components, for which only single excursions were necessary. Although previous measurements had been made on the pure components, they were here repeated in the interest of greater uniformity.

Two sorts of information are given by the measurements. In the first place there are the volume discontinuities at the various transitions. Pure bismuth has two transitions in the general neighborhood of 25,000 kg/cm² with volume changes of roughly 4 per cent each. These transitions of the pure bismuth are also exhibited by the solid solutions of small tin content. The volume change associated with this transition diminishes as the total bismuth content becomes less through the series, but the pressures of the transitions experience no measurable change. These two transitions are so close together that they usually appeared as one with the present apparatus, being resolved only under especially favorable circumstances. In addition to the volume discontinuities to be ascribed to the bismuth transition, there is the volume change accompanying the formation of the intermetallic compound out of the virgin eutectic mixture. This is irreversible and usually does not occur until the pressure of the bismuth transition is nearly reached. It does not run sharply and often is running concurrently with the bismuth transition, which tends to obscure the results. Finally, there is the transition between the high and low pressure modifications of the compound. The transition from the high to the low pressure modification runs rapidly and apparently completely, but only with considerable overshooting of the pressure mark. One of the tasks of the measurements is to get the best parameters for these various transitions, allowing as well as possible for the fact that equilibrium is seldom if ever completely attained.

In addition to the volume discontinuities, there are the changes due to ordinary compressibility in the ranges where there are no discontinuities. It is a task of the measurements to get the volume compressibilities in the continuous ranges. This includes the initial virgin material and the various pressure modifications in their various mixtures with each other.

A. *The volume discontinuities at the transitions.* There is some uncertainty in disentangling the various transitions. Consider, for example, the case of volume creep toward smaller values on the first application of pressure at 23,000 kg/cm². Is this creep to be

ascribed to formation of the compound at a pressure beyond
the equilibrium pressure between the two modifications of the
compound, or is it to be ascribed to premature formation of the
high pressure modification of bismuth, the normal equilibrium
pressure having been lowered by the presence of tin in solution?
It would perhaps have been impossible to obtain a satisfactorily
consistent interpretation of all the transition phenomena if only
the volume measurements were available, but fortunately the re-
sistance measurements are also available, and the relations of the
resistances are such as to remove possible ambiguities. When a
transition runs because of increasing pressure the volume, of
thermodynamic necessity, decreases, but the electrical resistance
may either increase or decrease. Now the formation of the com-
pound from the eutectic proves to be accompanied by an increase
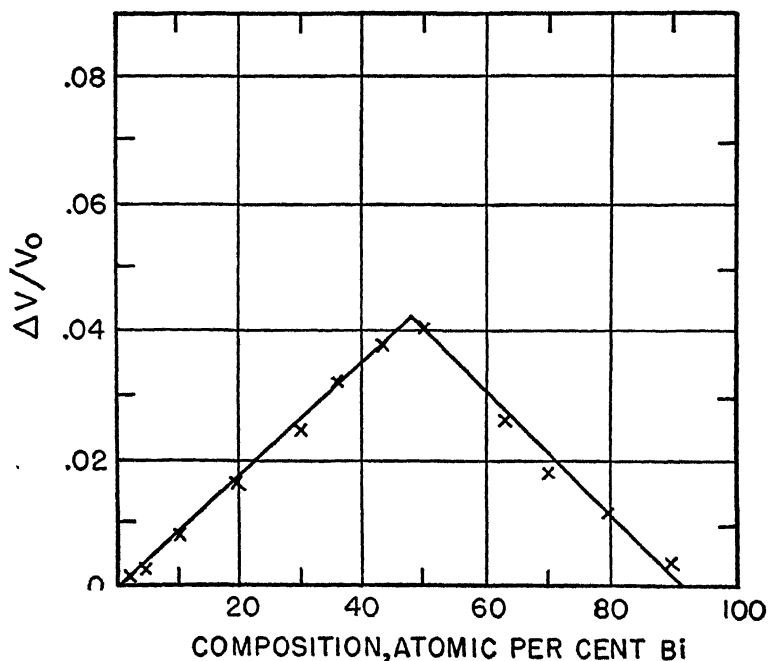

Figure 2. The discontinuity of volume when the high pressure modification
of the compound, C II, forms from the virgin eutectic, as a function of
composition.

of resistance, whereas the first high pressure transition of bismuth is accompanied by a large decrease of resistance. If then the creep of resistance at 23,000 is in the direction of an increase it means compound formation, whereas if it is a decrease it means the bismuth transition. It may be anticipated to say that it was always compound formation at this pressure; the normal transition pressure to the high pressure form of bismuth is not appreciably depressed by the tin in solution.

In Figures 2, 3, and 4 the several volume discontinuities are shown as a function of composition. Three discontinuities are to be considered: (1) the discontinuity when the high pressure modification of the compound (hereafter to be designated as C II) is formed from the virgin eutectic, shown in Figure 2; (2) the volume discontinuity accompanying the bismuth transition, which
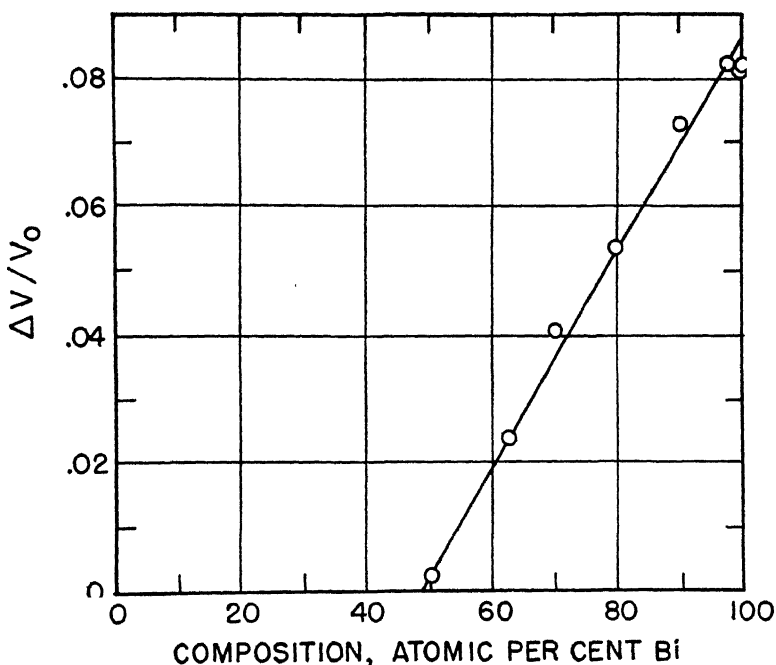

Figure 3. The discontinuity of volume accompanying the transition to the high pressure form of bismuth, Bi III, through the intermediate Bi II, as a function of composition.
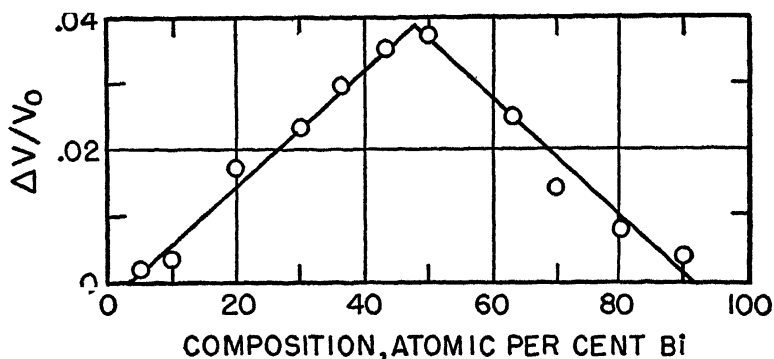
Figure 4. The volume discontinuity in the bismuth-tin system when the low pressure form of the compound, C I, changes to the high pressure form, C II, as a function of composition.

is actually two transitions, I-II and II-III together, being too close for resolution, shown in Figure 3; and (3) the discontinuity when the low pressure modification of the compound changes to the high pressure form, or vice versa, that is, for the transition C I to C II, shown in Figure 4.

Three independent measurements of (2) are possible: on first release, second increase, and second decrease of pressure. The average of the three is shown in the figure. The curves were so drawn through the observed points as to locate this transition at 25,000 kg/cm², very approximately the mean pressure from previous determinations. The mean pressure of the two transitions is actually 26,360 instead of 25,000 kg/cm². The difference between 25,000 and 26,360 involves an error in the volume discontinuity equal to the difference of compression of the two bismuth modifications over the pressure range of 1,360 kg/cm². This error is negligible.

Three independent measurements of (3), the volume difference between C I and C II, are also possible and on the same pressure excursions as for the bismuth transition. There is considerable sluggishness with respect to this transition, identical values for the volume not being obtainable with increasing and decreasing pressure without impractically long waiting, and not certainly even then. The discontinuities were usually located at 20,000

kg/cm² with increasing pressure and at 15,000 with decreasing. A negligible error due to difference of compressibility of the two forms is associated with this difference of 5,000. In general the transition with decreasing pressure ran more abruptly once it had started than the transition with increasing pressure.

The discontinuity (1) was given by only a single measurement for each set-up with each composition. This change runs so sluggishly that it was not possible to completely isolate it from the bismuth transition. Accordingly, the total discontinuity, bismuth transition plus volume change of formation of C II from the eutectic, was determined at 25,000 on the initial increase of pressure and the part due to formation of C II obtained by subtracting from the total the part due to the bismuth transition as already obtained under (2) as the mean of three measurements.

The discontinuities could not always be as cleanly analyzed as would be suggested by the description just given, but occasionally small satellite discontinuities appeared in addition to those just described. These are probably due to unusually large and capricious overshooting of the nucleation of the transition in isolated regions. In addition to this type of thing, there were a few instances where a small amount of the bismuth transition appeared at compositions where ideally it should not be present, namely at compositions with less than 50 At per cent bismuth on the second application of pressure. This is probably to be explained by a small reversion of C I to the eutectic during the lower part of the previous pressure excursion. It is known that C I reverts on standing at atmospheric pressure at room temperature, and there is no apparent reason why reversion should not take place more slowly at pressures higher than atmospheric.

Turning now to a consideration of Figures 2, 3, and 4, all three figures show a marked episode at approximately 50 At per cent bismuth. The bismuth transition does not run at all until the composition has reached 50 per cent bismuth; from this composition on the volume discontinuity increases linearly with the bismuth content. The interpretation is obvious. At less than 50 per cent bismuth all the bismuth is used up in pairing with atoms of tin to form the compound BiSn. Above 50 per cent bismuth there is an excess of bismuth after all the tin atoms have been paired with bismuth, and it is the excess bismuth which undergoes the

transition. The same interpretation applies with respect to the compound. The amount of compound present is a maximum at 50 At per cent, and on each side of this composition the amount of compound diminishes linearly with the amount of that component which is present in smaller amount. The linear relation between various volume discontinuities and composition indicates that in the aggregate there is little if any reaction between the microscopic grains, which probably take the form of a mechanical mixture without mutual solubility. This is consistent with the fact that the pressure of the two types of transition is constant, independent of composition. The principal argument for a compound of 50-50 composition is that the breaks in the volume discontinuities occur at this composition.

It will be noticed that the volume discontinuity shown in Figure 2 for compound formation is slightly greater than that in Figure 4 for compound transition. This means that at the pressure of equilibrium between C I and C II the compound is formed from the eutectic with slight loss of volume. Whether the volume relations are the same at atmospheric pressure depends on the relative compressibility of eutectic and C I over the 15,000 kg/cm² range back to atmospheric. It is obvious that if the density difference between eutectic and C I at atmospheric pressure really varies in the manner shown in the figure there must be slight departures from linearity between compressibility and composition.

B. *The volume compressions in the continuous pressure ranges.* The experiments disclosed four different sorts of continuous pressure range. There is in the first place the virgin material composed of a eutectic mixture of saturated dilute solution of tin in excess bismuth and a dilute saturated solution of bismuth in excess tin. This compresses uniformly up to the pressure of irreversible appearance of the high pressure modification of the compound C II. It must be recognized that in addition to the pure volume compressions under pressure there may be changes in the saturation limits with pressure. These changes would appear from these experiments to be small and they will be neglected. This first continuous pressure range on the virgin material terminates at between 20,000 and 25,000 kg/cm². The second continuous range also terminates at its lower edge at atmospheric pressure and embraces the low pressure modification of the compound, C I, and
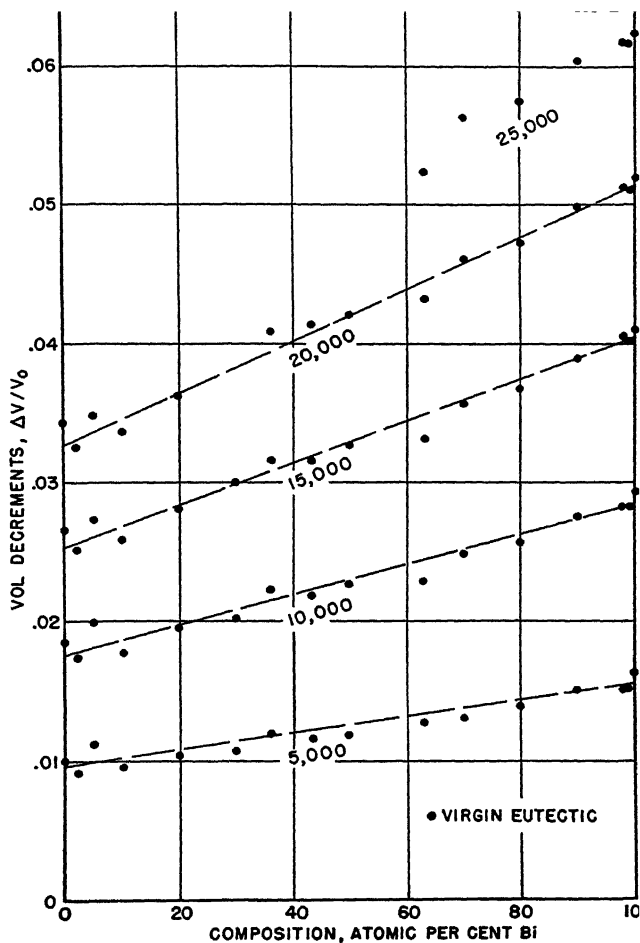
Figure 5. The volume compressions to the indicated pressures as a function of composition of the virgin eutectic in the bismuth-tin system.

either approximately pure tin or pure bismuth, depending on which side of 50-50 the gross composition lies. The upper limit of this range is 15,000 on release of pressure and 20,000 on increase. The third continuous range applies to the high pressure modification of the compound, C II, and either approximately pure tin or pure bismuth. Readings in this range can be obtained only with decreasing pressure between 25,000 and 15,000. Finally, the fourth
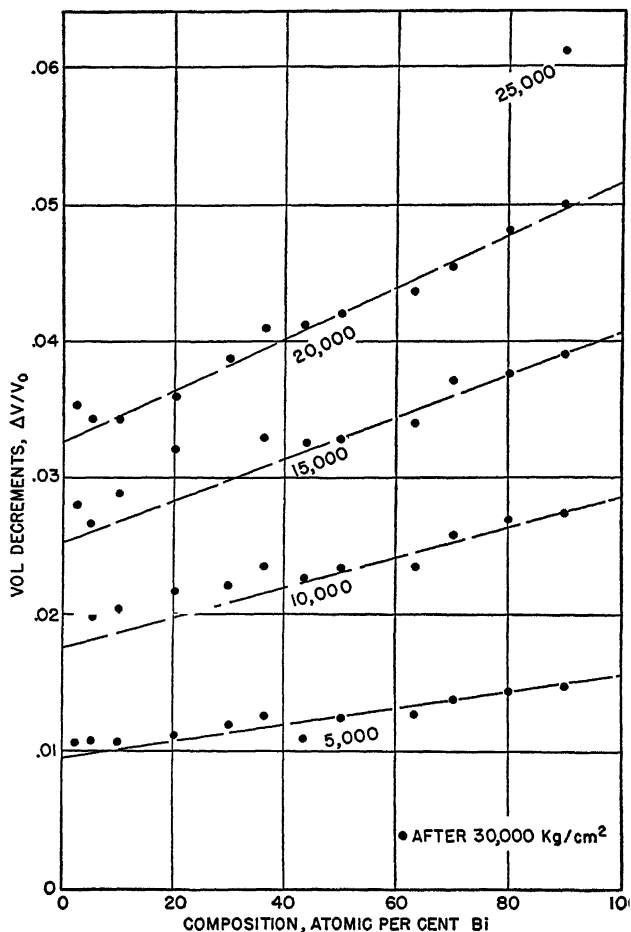
Figure 6. The volume compressions to the indicated pressures as a function of composition after exposure to pressure and irreversible formation of the low pressure modification of the compound, C I.

continuous range applies to C II and either approximately pure tin or bismuth. The range occurs above 25,000. For those compositions in which one of the constituents is approximately pure bismuth the bismuth exists in the form Bi III. Four sets of measurements can be made on this range: increasing or decreasing pressure on the two excursions to the maximum pressure.
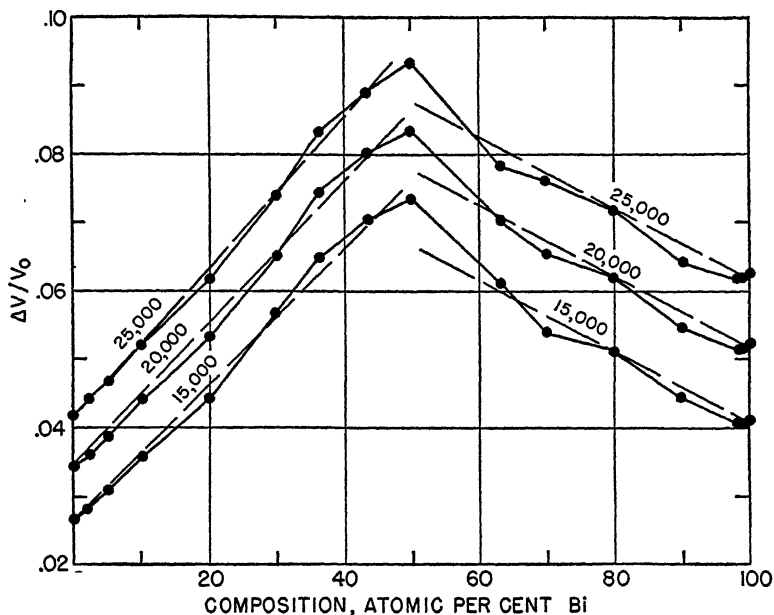
Figure 7. The volume compressions to the indicated pressures as a function of composition of the bismuth-tin system in that pressure range in which one of the components is the high pressure form of the compound, C II, and below the pressure of the appearance of the high pressure phases of bismuth.

Figures 5, 6, 7, and 8 show the volume compressions from atmospheric pressure for the four continuous ranges at 5,000 kg/cm² intervals of pressure as a function of composition. The "volume compressions" shown are $\Delta V/V_0$, where V_0 is the volume at atmospheric pressure of virgin material of the particular composition. If one wants to find the volume changes for systems of different compositions made up of the same total number of atoms, correction factors will have to be applied involving the initial density and the atomic weights.

The most striking feature of the four figures is that to a rough first approximation they are either linear in composition or are composed of two straight lines (Figure 7). This means that the volume decrements for any of the four ranges can be calculated from the corresponding volume decrements of the extreme compositions by simple linear interpolation. The volume decrements
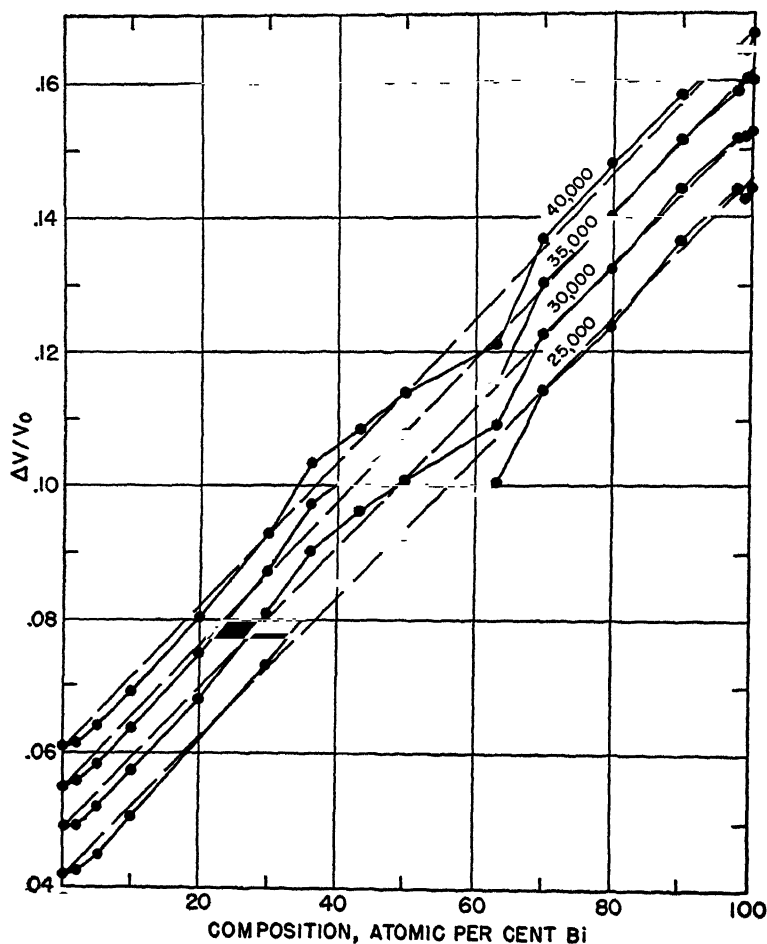
Figure 8. The volume compressions to the indicated pressures as a function of composition of the bismuth-tin system in the upper pressure range above the various transitions.

necessary in such a computation are given in Table II. It is to be emphasized that this table is to be used only for the purpose for which it was constructed, that is, to give fairly good approximations for the compressions over the entire composition range. It is highly probable that some at least of the deviations from linearity shown in the figures is real, but to establish this with any security

would demand extensive further experimenting. In particular, the table does not purport to give the best values for the compressions of the pure metals at the extremes of composition. For these, better values are at hand. It is merely that this table gives a summary for the intermediate compositions which is not far from the

TABLE II*

Volume Decrements, $\Delta V/V_0$

Pressure kg/cm²	Pure Bi	Pure Sn	Compound II
5,000	.0155	.0095	
10,000	.0285	.0175	
15,000	.0405	.0253	.0667
			.0760
20,000	.0514	.0326	.0778
			.0862
25,000	{ .0620	.0415	.0875
	{ .1455		.0960
30,000	.1535	.0490	
35,000	.1615	.0550	
40,000	.1676	.0605	

*See text for method of using this table.

present experimental error. The deviation from linearity is obviously greatest for the volume decrements of C II. For compositions not 50 per cent, a better approximation is obtained by using two straight lines on the two sides of 50 per cent with two different values for the volume decrement of C II. Of the two values given in the table for each pressure, the upper is to be used on the bismuth rich side of 50 per cent.

It was to be expected that the volume decrements above 25,000 should be given by *two* lines, meeting at a cusp, as for the system including C II between 15,000 and 25,000. For obviously the decrements for pure tin in this pressure interval do not include any phase transitions, whereas on the pure bismuth end the volume decrements include those of the phase transitions as well as the compressions above and below the transition. At 50 per cent composition the system is pure C II, and the corresponding volume decrement should be the volume discontinuity at the transition of C I to C II and the pure volume compressions of C I and C II below

and above the transition pressure. The fact that a single straight line instead of two reproduces the results means that the volume decrement just described associated with C II is the mean of the two decrements of pure tin and pure bismuth. This is a surprising relation. It means that in the compound each atom of bismuth or tin occupies approximately the same volume that it would occupy if that atom were seated in its own massive metal under the actually prevailing external pressure and *in the appropriate state of modification*. This simple statement is of course to be qualified by the second degree terms that appear on working out the analytic reduction from fractional changes of volume to actual number of atoms present, but the second degree terms are small and do not importantly modify the simple picture.

In line with the simple state of affairs disclosed in the last paragraph is the fact that the compressions of Figures 5 and 6, that is, the compressions for the system with virgin eutectic and for the system including C I, are approximately the same. This means that not only does C I form from the components with little change of volume, but that the compressibilities are also additive, something which evidently implies a higher degree of lack of interaction between the atoms than does the mere volume change on formation of the compound.

It is evident from the figures that the simple linear relations just discussed should be taken only as first approximations. It does not appear to what extent an attempt to obtain more refined information would be justified. There do seem to be certain consistent irregularities at the ends of the series in the neighborhood of the pure components. These may be correlated with the solid solutions which are supposed to exist at the two ends. The solubility limit of tin in bismuth is supposed to be only one or two per cent, whereas that of bismuth in tin has been given as much as 10 per cent. A difference in this direction in the solubility limits is suggested by Figure 5. Except for the one point at 5 At per cent bismuth, it would seem that the compressibility of either dilute solution is depressed compared with that of the pure metal. Most of the compressions associated with 63 At per cent bismuth in the figures are out of line by more than appears probably due to experimental error, and usually in the direction of too small a compression. No consistent correlation appears between deviation

from linearity in the figures and the permanent changes of density measured at atmospheric pressure after exposure to pressure. Clearing up these points would demand much more elaborate measurement.

Electrical Resistance. Measurements were made of the specific resistance at atmospheric pressure, of the temperature coefficient at atmospheric pressure, of the effect of pressures to 30,000 kg/cm² at room temperature, of pressure to 100,000 at room temperature, and various fragmentary studies of the secular changes taking place in the resistance after exposure to pressure in virtue of the irreversible transformations induced by pressure.

The material was extruded to wire of 0.020 inch diameter for the measurements, which were made by the potentiometer method with four leads. It might perhaps be feared that extrusion would introduce mechanical imperfections, such as incipient cracks. If so, there should be internal evidence of it in an abnormally rapid change of resistance for the first small increments of hydrostatic pressure. No such effects were ever found. Two sets of specimens were prepared, one for the measurements under pressure and the other for the various measurements at atmospheric pressure. For the pressure measurements the terminals were attached by soldering with ordinary lead-tin solder. For the measurements at atmospheric pressure the specimens were mounted in a jig with four spring contacts at fixed distances apart. By using a jig, all the atmospheric specimens were measured under standard conditions, which could be repeated after the lapse of time and exposure to temperature variations which might have loosened any soldered contacts. Certain irregularities in the potentiometer readings due to variable and high resistance at the spring contacts were eliminated by passing the current from a small magneto through the contacts before the readings.

A. *Specific resistance at atmospheric pressure.* Specific resistances at atmospheric pressure were measured for 20 different compositions. These measurements were made at room temperature, at 0°C, and then back at room temperature again. The check reading at room temperature was made in order to be sure that no internal change had been induced by the temperature change. None such was found. From these readings were calculated both the specific resistance at 0°C and the mean temperature coefficient

between 0° and room temperature in terms of the resistance at 0°.
In calculating the specific resistance the mean diameter of the wire
was used as determined by six micrometer readings to 0.0001 inch.
The wires were fairly uniform and any error in specific resistance
from uncertainty in dimensions should be a fraction of one per
cent. After these measurements, all twenty wires were exposed
simultaneously to 30,000 kg/cm², a pressure high enough to induce
complete transformation to the compound. The wires so exposed
were then remeasured for specific resistance and mean temperature
coefficient between 0° and room temperature. In order to mini-
mize any internal changes during the measuring process all wires
were kept at dry ice temperature except during the actual opera-
tion of measurement, which might consume some twenty minutes.
The specimens were then stored in dry ice, and remeasured after
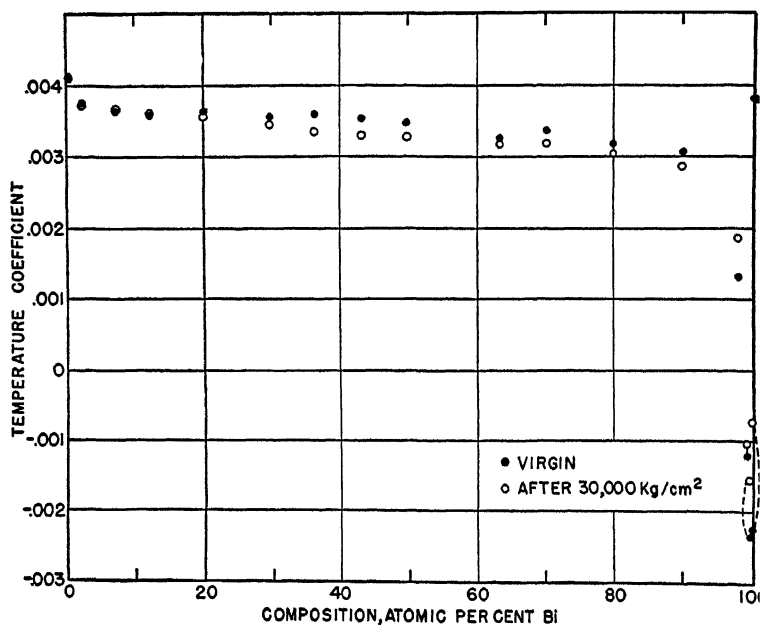the lapse of two weeks for secular changes. They were then kept

Figure 9. The specific resistance at atmospheric pressure at 0° C of the
bismuth-tin system as a function of composition, except for dilute solutions
of tin in bismuth, before and after exposure to pressure.

at 100°C for 12 hours and remeasured at room temperature for further secular changes.

The specific resistances at 0° at atmospheric pressure are shown as a function of composition in Figures 9 and 10. The first addition of tin to pure bismuth is seen to result in a large increase of resistance, amounting to nearly sixfold at approximately one per cent tin.
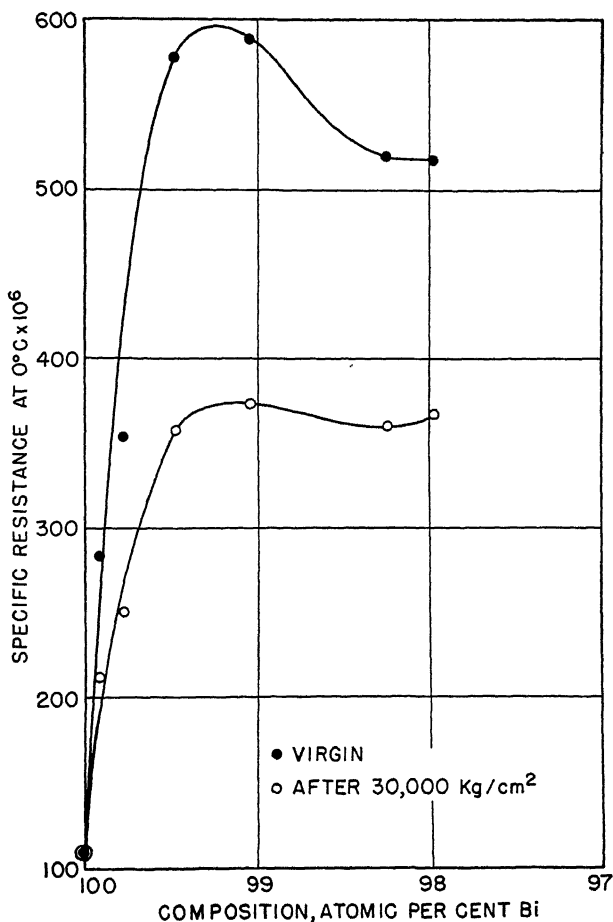

Figure 10. The specific resistance at atmospheric pressure at 0°C of the bismuth rich concentrations of the bismuth-tin system before and after exposure to pressure.

The specific resistance of this series has also been measured by Thomas and Evans.[5] The agreement on the tin side up to 50 At per cent tin could perhaps be called satisfactory, but between 50 and 100 per cent bismuth there are wide discrepancies, for which the explanation does not appear. What is worse, the sign of the discrepancy varies. At 90 per cent bismuth their specific resistance is 275×10^{-6} against my 142×10^{-6}, whereas their maximum occurs at 98 per cent with a specific resistance of 393×10^{-6} against my maximum at about 99 per cent with a specific resistance of 588×10^{-6}.
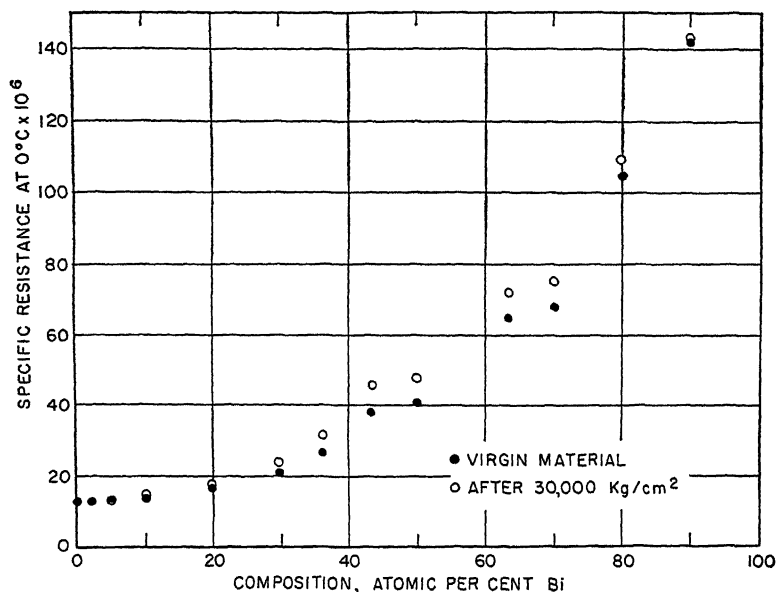
Figure 11. The mean temperature coefficient of resistance between 0°C and room temperature of the bismuth-tin system as a function of composition, before and after exposure to pressure.

Discussion of the various changes of resistance after exposure to pressure will be deferred until after presentation of the pressure effects.

B. *Temperature coefficent of resistance at atmospheric pressure.* The temperature coefficient of resistance between 0°C and room temperature is shown in Figure 11. It will be seen that the first

small additions of tin to bismuth result in a very rapid depression of the temperature coefficient, with change of sign to a minimum of −0.00237 at 0.2 At per cent Sn, followed by a rapid recovery to positive values, which remain between 0.003 and 0.004 over the entire range of composition from 10 per cent tin to pure tin. It is to be noticed that the minimum temperature coefficient occurs earlier than the maximum of specific resistance, namely at 0.2 per cent against approximately one per cent.

The temperature coefficient between 0° and 30° has also been given by Ufford.[4] He too finds the reversal of sign at a small concentration of tin, but the numerical agreement is not good. The smallest coefficient which he found was −0.00044 at 0.53 At per cent tin. There seem to have been considerable experimental irregularities in his work, because the curve which he draws for the best temperature coefficient fails by a rather wide margin to pass through all the experimental points. Doubtless the explanation of the most important part of the discrepancy at low tin concentrations is insufficient purity in Ufford's bismuth. His alloys were made up with Kahlbaum " K " bismuth, and his compositions are given in terms of it. The temperature coefficient of this was only 0.0028 against 0.00383 for the purified bismuth used in making the present series of alloys.

C. *Effect of pressure up to 30,000 kg/cm² on resistance.* Measurements were made on 26 different specimens, including five 24-year-old specimens left from the work of Ufford. The latter had been kept sealed in glass under nujol. The pressure phenomena are highly complex, and, short of reproducing all the original data, can be described only with a certain amount of conventionalization. There are several regions of unusual effects. In the first place, the region of dilute solution of tin in bismuth is unusual in the nearly sixfold increase in resistance accompanying only one per cent of tin. The resistance in this region is to be thought of as the resistance of pure bismuth plus an " additive " resistance due to the foreign atoms of tin. This additive resistance appears to be abnormally sensitive to pressure, being in general decreased by it, so that the total pressure effects in this region exhibit reversals of curvature and points of inflection. One sort of phenomenon which might have been anticipated in this region did not manifest itself however; namely, changes in the solubility limits produced by

pressure accompanied by solution or unsolution effects with corresponding creep of the resistance. No creep effects were found on the first application of pressure to the virgin alloy until a transition pressure had been reached, and all creep effects, of which there were many, appeared to be associated with one or another of the transitions. These transitions were the same as those already met in the volume measurements. First is the irreversible formation of the high pressure modification of the compound on first increase of pressure. In general, resistance increases during this change except at low tin concentrations where the additive resistance is high. Here, resistance decreases on formation of C II because of disappearance of the high additive resistance. The irreversible formation of C II takes place, except at low tin content, throughout the region 20,000 to 25,000 kg/cm^2 and is sluggish and pressure sensitive. That is, one may wait at, say, 23,000 kg/cm^2 a sufficient time for the reaction to apparently complete itself, and then on increasing pressure to 24,000 the reaction runs some more. At low tin content, below 30 per cent, C II forms only simultaneously with Bi II, that is, above 25,000 kg/cm^2.

Next, on increase of pressure, there is the transition to Bi II. This runs with large decrease of resistance, sixfold or more for pure bismuth, and is always a comparatively clean-cut and rapid reaction, being completed within a pressure interval of the order of 1,000 kg/cm^2.

Next, on increase of pressure, occurs the transition from Bi II to Bi III, accompanied by an increase of resistance, which for pure bismuth is two or threefold. Because of the reversal of direction of the changes of resistance it was easy to resolve the two transitions of bismuth by means of the resistance measurements, although they were usually fused together for the volume measurements. On release of pressure the two bismuth transitions may with care be found in the inverse order. The transition III to II, however, has a strong tendency to overshoot on release of pressure and may not occur until the transition pressure between II and I is passed. The appearance then is as if there were direct passage from III to I. Whether there is actually intermediate formation of II would be difficult to establish, but the probability is in favor of it because creep has been observed in both directions in this region. The region of several thousand kg/cm^2 below 25,000 on release of

pressure is likely to be a region of creep, and this region may extend so far that the creep begins to be mixed up with the transition from C II to C I. This transition also runs throughout a region with no clean-cut limits. The speed of the transition tends to become greater at lower pressures and increases so rapidly that there is a sweep in the neighborhood of 15,000. Below 15,000 the transition is completed, creep disappears, and the system under measurement consists of so much of C I as is yielded by the gross composition plus approximately pure tin or pure bismuth as the case may be.

Added to all the complications due to phase changes there are smaller complications arising from the non-cubic nature of the pure components and presumably of the alloys, with corresponding non-isotropy of resistance. This means that on passing back and forth through a transition the original resistance will not be recovered unless the original orientation is recovered. It is known that transitions may be divided into two classes according as the original orientation is recovered or not. Orientation is not recovered in the bismuth I-II transition, so that resistance measurements on pure bismuth in the region below 25,000 are in general not completely reversible after an excursion above 25,000, although the failure is usually small. The irreversibility is plainly apparent in the appearance of the wire after a pressure excursion, which is full of small roughnesses and offsets. After a cycle with transition there was in general a permanent change of length, usually a decrease, by an amount seldom if ever greater than one per cent. In computing the permanent change of specific resistance after a pressure run allowance was made for the permanent change of length, assuming no permanent change of density.

The II-III transition of bismuth, unlike the I-II transition, appears to be reversible with respect to orientation. It would appear that the C I-C II transition is not reversible with respect to orientation, because on passing back and forth through the transition the resistances do not repeat. However, these effects are small, only a few per cent on total resistance.

In general the creep effects in the resistance measurements were much more capricious than in the volume measurements. This is probably associated with the difference in geometrical shape of the specimens, a massive hunk in the one case and a long slender

TABLE III
Specific Resistance × 10⁶ of Bismuth-Tin Series

Pressure Kg/Cm²	0		5,000		10,000		15,000	
100% Bi	109.00		119.78		131.94		145.63	
	Increase	Decrease	Increase	Decrease	Increase	Decrease	Increase	Decrease
99.916% Bi	235	213.1	271.1	232.6	307.8	249.0	339.2	262.7
99.779% Bi	367.0	292.6	408.2	303.7	438.5	309.0	456.1	311.6
99.485% Bi	509.	331.9	524.0	326.6	522.0	318.4	508.8	308.4
99.051% Bi	588	373.2	576.0	358.1	552.4	343.9	524.0	326.0
99.051% Bi†	599.6	433.0	581.4	413.2	555.5	393.8	523.8	371.1
98.25% Bi	582.0	397.0	551.6	377.5	514.6	355.6	476.8	331.3
97.98% Bi	551.6	400.5	533.5	383.3	507.4	365.4	478.8	{346.7 / 343.7
95% Bi*	266.5	240.9	257.6	230.8	248.7	219.8	238.4	210.2 / 193.2}
90% Bi*	151.6	141.0	145.5	135.1	139.3	129.2	133.3	123.2
89.88% Bi	203.6	195.6	192.7	186.7	181.9	176.9	172.2	167.5
80% Bi*	104.5	105.5	99.8	100.7	95.5	96.2	91.5	92.1
79.89% Bi	122.6	155.4	116.4	148.0	110.3	139.7	104.7	{132.5 / 113.0
70.05% Bi	68.2	88.00	64.6	83.68	61.28	79.52	58.34	76.20 / 63.2}
63.27% Bi	71.9	85.32	68.1	81.6	64.7	77.9	61.6	{74.4 C I / 63.3 C II
49.8% Bi	44.75	58.20	42.50	55.72	40.50	53.25	38.70	51.00}
43.4% Bi	33.84	45.32	32.30	43.12	30.97	41.16	29.68	{39.47 / 32.34
43.4% Bi†	33.84	44.20	32.27	42.30	31.02	40.51	29.73	38.76 / 32.21}
36.2% Bi	23.87	26.80	22.88	25.72	21.99	24.71	21.16	{23.90 / 21.98
29.72% Bi	24.47	28.40	23.40	27.14	22.48	26.04	21.68	25.16}
19.99% Bi	16.28	17.95	15.60	17.20	15.00	16.53	14.46	{15.93 / 15.12
10.08% Bi	15.08	15.68	14.49	15.03	13.93	14.46	13.41	13.94 / 13.70}
5.07% Bi	13.93	14.10	13.36	13.50	12.85	12.97	12.43	12.48

	Increase-Decrease	Increase-Decrease	Increase-Decrease	Increase-Decrease
2.05% Bi	14.11	13.55	13.00	12.52
100% Sn	13.520	12.852	12.277	11.774

† 2nd specimen.
* Ufford's specimen.

TABLE III
SPECIFIC RESISTANCE × 10⁶ OF BISMUTH-TIN SERIES

20,000		25,000		30,000	Pressure Kg/Cm²
160.66		178.66		23.9 II at 26,000 / 98.2 III at 27,000	100% Bi
Increase	Decrease	Increase	Decrease	Increase	
367.8	276.7	392.8 / 28.4 II	286.1	75.0 III at 27,000 / 73.4 III at 30,000	99.916% Bi
467.9	314.1	474.0 / 28.3 II	316.3	75.2 III at 27,000 / 73.6 III at 30,000	99.779% Bi
487.2	297.0	460.0 / 28.4 II	286.0	75.6 III at 27,000 / 74.5 III at 30,000	99.485% Bi
491.4	308.0	455.5 / 28.6 II	288.9	75.8 III	99.051% Bi
491.2	349.0	457.3 / 30.2 II	323.7	78.0 III	99.051% Bi†
437.7	302.6	27.9 II		73.0 III at 27,000 / 72.6 III at 30,000	98.25% Bi
447.8	327.2	415.8 / 29.5 II	310.5	74.7 III at 27,000 / 74.0 III at 30,000	97.98% Bi
229.7	183.6	219.8 / 27.0 II	175.2	65.0 III at 27,000 / 64.3 III at 30,000	95% Bi*
127.6	116.7 / 112.7?	23.2 II	106.8	49.2 III at 27,000 / 48.2 III at 30,000	90% Bi*
163.5		30.7 II			89.88% Bi
88.2		85.3 / 27.2 II		46.6 III at 27,000 / 45.8 III at 30,000	80% Bi*
99.9 / 106.8	106.8				79.89% Bi
55.40 / 59.86	73.02 / 60.0	57.15 / 31.80 II		42.50 III at 27,000 / 41.77 III at 30,000	70.05% Bi
58.8 / 60.90 C II		58.48 C II / 30.61 II			63.27% Bi
				34.04	49.8% Bi
28.40 / 30.72		29.64 / 28.30		27.89	43.4% Bi
28.98 / 31.07		30.00		29.14	43.4% Bi†
20.50 / 21.23		20.52		19.93	36.2% Bi
20.97 / 21.40		20.48		19.80	29.72% Bi
14.03 / 14.54		14.00		13.52	19.99% Bi
12.95 / 13.14		12.68		12.26	10.08% Bi
11.99	12.04	11.65		11.23	5.07% Bi
Increase-Decrease		Increase-Decrease		Increase-Decrease	
12.09		11.70		11.34	2.05% Bi
11.302		10.887		10.509	100% Sn

† 2nd specimen.
* Ufford's specimen.

wire in the other. With the wire it would appear to be easier for the transition in one part of the wire to get isolated from the rest, so that completion of the transition throughout the whole wire might require several independent nucleations.

A complete summary of the experimental results on all specimens, smoothed and conventionalized at 5,000 kg/cm² intervals, is given in Table III. The many gaps in the table correspond to regions with too much creep to justify unique values. Selected results from the table are shown in Figures 12, 13, and 14. Figure 12 is for the middle of the very dilute tin range, where specific
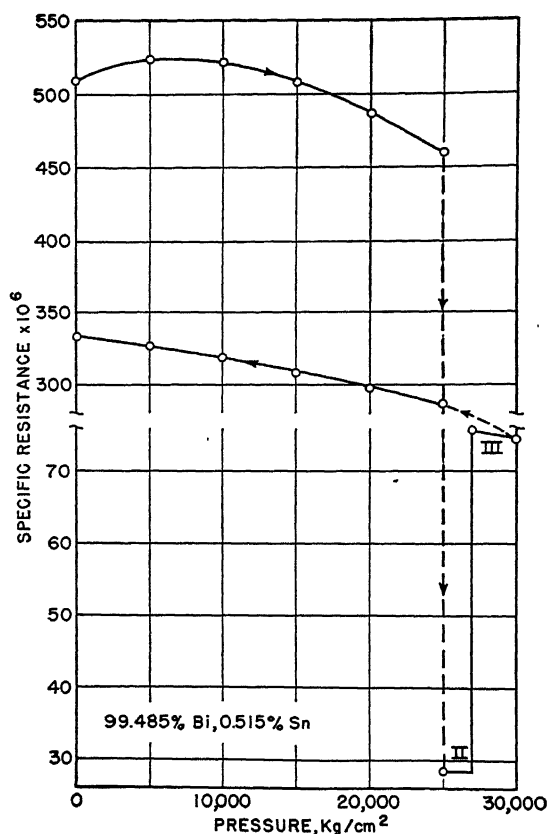
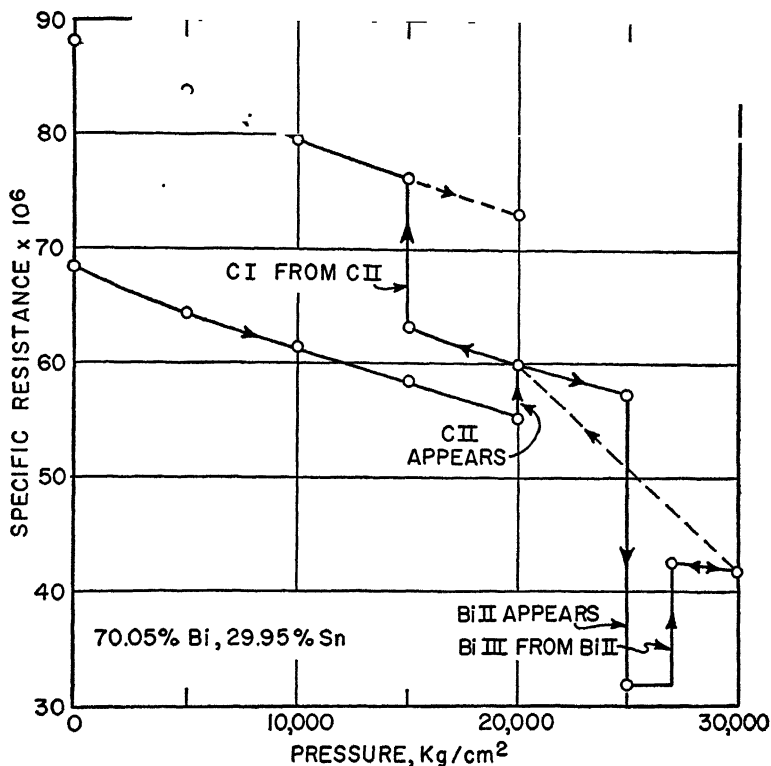Figure 12. The specific resistance as a function of pressure for a dilute solution of tin in bismuth.

Figure 13. The specific resistance as a function of pressure for a bismuth-tin alloy for a composition showing all the transitions.

resistance is high and very sensitive to composition. Figure 13 is in the middle of the bismuth rich end where the two bismuth transitions dominate, and Figure 14 is in the middle of the tin rich end, where the bismuth transitions have dropped out and the formation and transition of the compound dominates.

In the following a verbal description will be given of the data presented in the table and figures. The descriptions will be grouped around the various homogeneous ranges.

First, consider the effects on the virgin material, before the entry of either bismuth II or C II, that is, the range up to 25,000 kg/cm² for smaller tin concentrations and to 20,000 for larger concentrations. We have already seen that the result of initial addition of
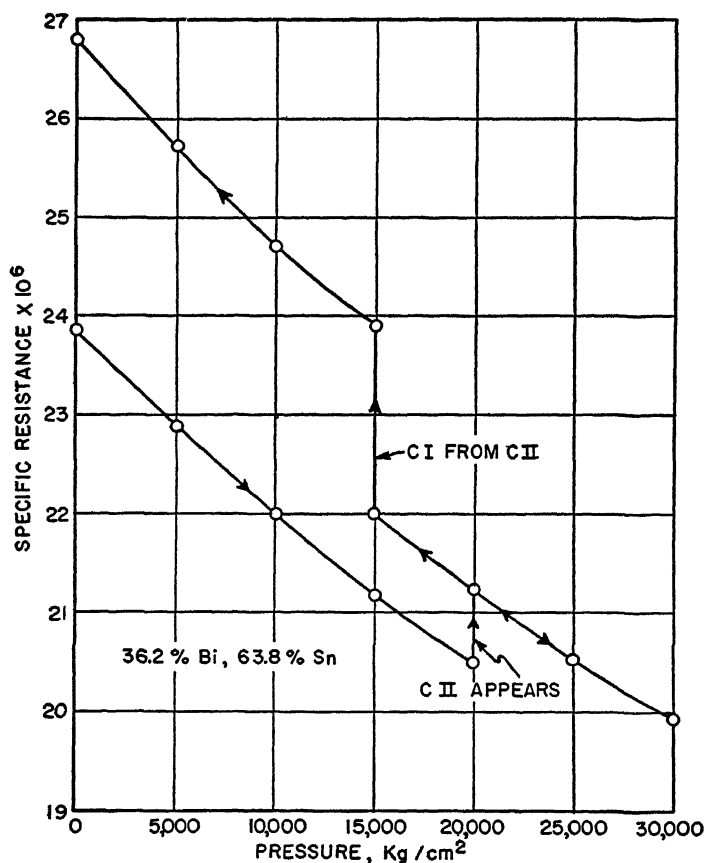
Figure 14. The specific resistance as a function of pressure for a bismuth-tin alloy for a composition in the tin rich range where the only transitions are those of the compound.

tin to bismuth up to one per cent tin is a very rapid rise of specific resistance to nearly 600×10^{-6} against 109×10^{-6} for pure bismuth. The resistance of pure bismuth against pressure rises at an accelerated rate, that is, with upward curvature. The effect of the initial small addition of tin of 0.084 per cent is to more than double the specific resistance and also to greatly increase the initial pressure coefficient of specific resistance, although not by as large a factor as the specific resistance. Increase of resistance with pressure

at this composition is at first concave upward, but the curvature presently reverses, and for the range above 10,000 and up to 25,000 the increase of resistance is concave downward. This is a highly unusual kind of resistance change and few examples are known. With increasing tin content, and still in the region of increasing initial specific resistance, the initial pressure coefficient becomes less and the transition from upward to downward curvature is displaced to lower pressures so that presently the curve is concave downward from the beginning. At 0.5 At per cent tin, shown in Figure 12, the downward concavity has become so large that there is a resistance maximum at less than 10,000 kg/cm². On further increasing the tin content to the composition of maximum specific resistance at atmospheric pressure, resistance drops with increasing pressure from the beginning and still with abnormal downward curvature. With further increasing tin content specific resistance drops, the pressure coefficient becomes less and the curvature less. Presently a point of inflection with reversal of curvature appears at the highest pressures; this point of inflection works toward lower pressures with further increasing tin content and reaches the middle of the pressure range at about 10 per cent tin. At higher tin content the curve becomes normally concave upward over the entire pressure range. There is no special episode on the tin rich end of the series, and therefore no evidence from these resistance measurements of any solubility of bismuth in tin.

Consider next the first bismuth transition, from I to II. This dominates the situation on the bismuth rich end of the series, but disappears beyond 50 At per cent tin, all the bismuth being used up in forming the compound. At low tin concentrations the first transition encountered is the Bi I-II transition. Ideally this should be preceded by the formation of C II. At small tin concentrations the formation of C II would be difficult to detect, the amount being proportional to the tin content. But as the tin content increases one would expect to encounter the formation of C II in no unmistakable terms. As a matter of fact the formation of C II prior to the bismuth. I-II transition did not show up until 30 At per cent tin was reached. It would appear that C II forms much more readily from a matrix in which bismuth II is present than from one with bismuth I. When the Bi I-II transition does run it is to be presumed that formation of C II occurs simultaneously.
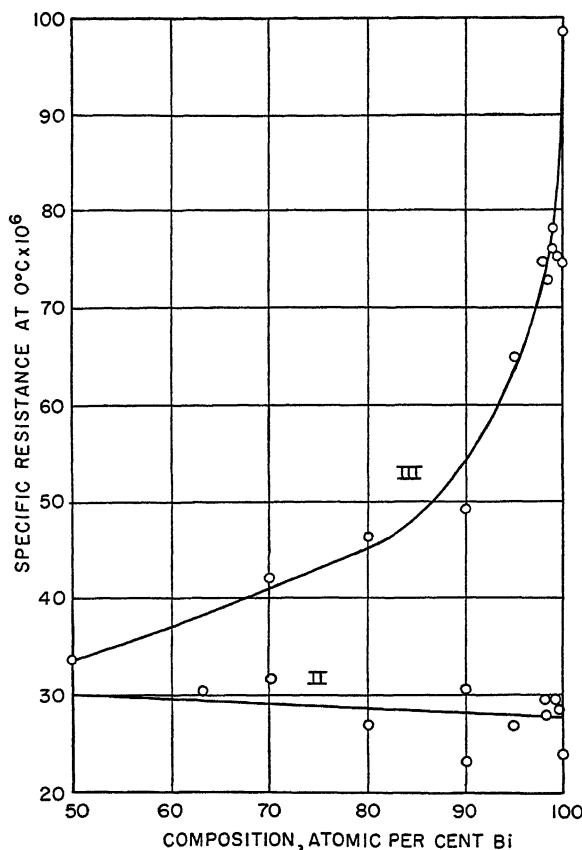
Figure 15. The average specific resistances above 25,000 kg/cm² for bismuth rich compositions in the bismuth-tin system in which one of the components is Bi II or Bi III.

In Figure 15 are shown the average specific resistances above 25,000 kg/cm² of the systems containing Bi II or Bi III. The system in the pressure region above 25,000 and below the Bi III transition at about 27,000 consists of Bi II and C II mixed in proportions corresponding to the gross composition. Now the figure shows that the specific resistance of this system is approximately constant over the entire composition range down to 50 per cent Bi. This constancy is attained in the face of a drop of resistance at

the transition from Bi I to Bi II by as much as a factor of 20 at low tin concentrations. Two probable conclusions would seem to be indicated by this. The first is that there are no solution phenomena of tin in bismuth II corresponding to the solution of tin in bismuth I which is responsible for the initial great increase of specific resistance. That is, one of the components of the system between 25,000 and 27,000 is probably *pure* bismuth II. The other conclusion is that the specific resistance of C II must be close to that of Bi II, for otherwise the resistance of the whole system would not remain constant during the change in the proportions of Bi II to C II from infinity to zero which occurs in the range of 50 per cent gross bismuth.

Above 27,000 kg/cm², where Bi III has formed, Figure 15 shows the specific resistance is not constant, but drops with increasing tin content from 98×10^{-6} to approximately the value of Bi II and C II. The explanation is consistent with that of the last paragraph. The specific resistance of Bi III is *not* the same as that of C II, but is several fold greater. Hence as the composition of the system varies from all Bi III to all C II the total resistance varies accordingly.

At tin contents higher than 30 At per cent the irreversible formation of C II usually occurs at a pressure markedly below the Bi II point, in the general neighborhood of 20,000 kg/cm². There is nothing sharp about the onset of this transition, either with respect to pressure or composition. On one occasion it was possible to make systematic measurements on it at a composition of only 10 per cent tin.

At 50 per cent tin the Bi II transition disappears and C II becomes the only transition with increasing pressure. The discontinuity of resistance accompanying the formation of C II becomes progressively smaller with diminishing bismuth content, since the amount of the phase C II formed becomes smaller. Evidence of the formation of C II was lost between 10 and 5 per cent bismuth. As already remarked, the formation of C II may be accompanied by an overall increase of resistance. It might appear at first as though there is here an inconsistency with the statement that the specific resistance of C II is approximately the same as that of Bi II. There is no inconsistency, however, when the absolute values of resistance are considered. The increase occurred only with

compositions where the tin content was so high that the total resistance of the system was less than that of pure Bi II.

On release of pressure from 30,000, the first transition encountered with the bismuth rich compositions was Bi III to Bi II. There was not so much interest in this transition and careful readings were not always made on it. Next was encountered the Bi II to Bi I transition. This was not the reverse of that with increasing pressure, because now the end product included as much of C II as was permitted by the gross composition. Below the Bi II point there was a range, extending down to the general neighborhood of 15,000, in which the system comprised C II plus the mixed Bi-Sn system. At 15,000, or over a range with 15,000 as the lower edge, the transition C II to C I occurred. This runs with increase of resistance. No very consistent values were obtained for the numerical magnitude of this increase because of the capriciousness of the transition between 20,000 and 15,000, hysteresis and delayed starting being by no means reproducible. A rough trend could be established, however. At the bismuth rich end of the series the increase of resistance of the whole system at the transition increases to a rough maximum in the neighborhood of 20×10^{-6} and 75 At per cent Bi, dropping to something of the order of 10×10^{-6} at 50 At per cent Bi. Here the system is presumably all in the state C I, so that the actual specific resistances are the specific resistances of pure C II and C I. These values were 40×10^{-6} for C I and 50×10^{-6} for C II, which are the presumptive values at 15,000 kg/cm^2.

The delay and creep phenomena in the region 15,000 to 20,000 are complex and often capricious; explanation along the comparatively simple lines suggested above could not always be defended against deliberate attack. However, no alternative explanation along more complicated lines seemed indicated with sufficient definiteness to take seriously. If ever this system is reexamined with greater elaborateness, the question which I believe should be examined most carefully is whether there are not three modifications of the compound in this region, with transition pressures so close that they cannot be easily resolved, analogous to the close transitions of pure bismuth.

Finally, below 15,000 on release of pressure, the system consists of so much of C I as permitted by the gross composition, plus

approximately pure bismuth or tin. Except at very low tin concentrations, up to about o.2 At per cent Sn, the pressure coefficient has the normal negative sign. In the very dilute region, there are reversals of direction and curvature analogous to those on initial increase of pressure, which can be evaluated in detail from the data in Table III. In the region of low tin concentrations, where the specific resistance is high, the specific resistance of the final system is less than that of the initial system. Between 10 and 20 per cent this reverses sign, and over most of the range the final system with C I has a higher specific resistance than the virgin system. As a function of pressure, the two curves, on initial application and final release, run nearly parallel, so that the pressure coefficient of the two systems is nearly the same.

The question arises as to what the other component is besides C I in the system after release of pressure. On the bismuth rich end it might conceivably be pure bismuth, but this seems unlikely, because in the region of high tin dilution even the final resistances are too high to be accounted for by any combination of pure bismuth and C I. It would seem probable that at high dilution most of the tin is removed from the original solution to form the compound, but that enough is left in solution at the saturation composition to bring the total resistance to the observed value. If the final system is assumed to consist of three components, saturated solution of tin in bismuth, compound, and saturated solution of bismuth in tin, then the distribution among the three components demanded by the phase rule is indeterminate. If, however, it is assumed that on the bismuth rich end there is no saturated solution of bismuth in tin, and on the tin end no saturated solution of tin in bismuth, then the distribution becomes determinate. Whether this is a plausible state of affairs or not could then be told by comparing the calculated resistance of such a system with the observed resistance. This calculation cannot be made without an assumption about the nature of the geometrical entanglement of the grains of the different components with each other. The question of the constancy of the method of entanglement would then arise. So far I have not attempted such a calculation.

The final system on releasing pressure containing C I is not stable with respect to the original system, at least at atmospheric pressure. The inverse reaction runs at room temperature with

observable velocity at atmospheric pressure, but in the natural time of observation of the experiments, of the order of ten minutes, no inversion was observed at pressures higher than 2,500 kg/cm². At atmospheric pressure the rate is so low at room temperature that the observations as obtained without special precautions should give with good approximation the resistance at atmospheric pressure of the system formed from C II at 15,000.

D. *Behavior of resistance at atmospheric pressure after exposure to pressure.* We return now to the topic left in abeyance on page 122. In Figure 16 are shown the ratios of the specific resistances at atmospheric pressure and room temperature before and after completion of a pressure cycle to 30,000 kg/cm² with irreversible formation of C I. The information in Figure 16 is contained in
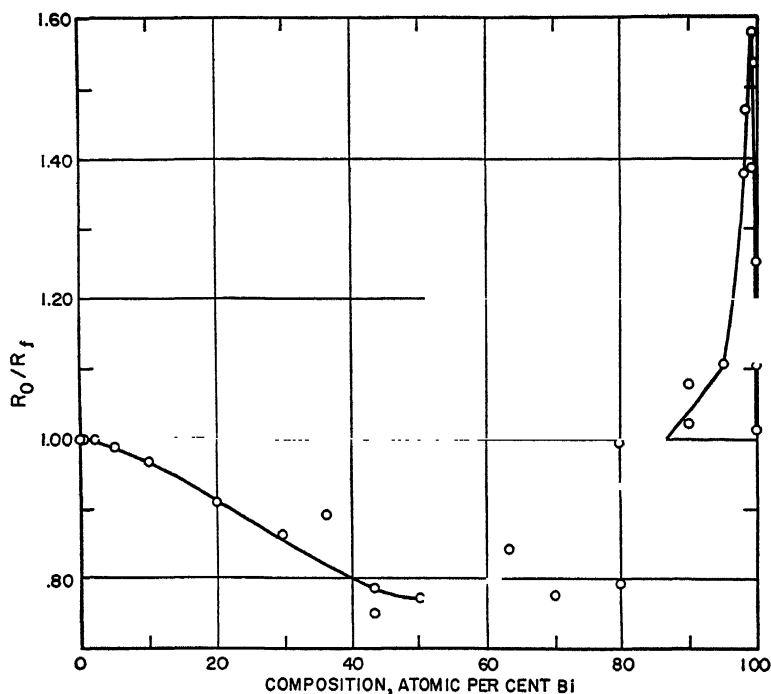

Figure 16. The permanent change in resistance, as a function of composition, in the bismuth-tin system after exposure to pressure. Essentially the same information, on another scale, is contained in Figures 9 and 10.

another form in Figures 9 and 10. There is considerable irregularity, but the general trend is unmistakable. At low tin concentrations there is a large decrease of resistance after the pressure cycle, rising to a maximum at 1 At per cent Sn where the specific resistance of the virgin material is also a maximum, dropping after the maximum to zero at about 10 per cent tin, followed by a more gentle increase which has its maximum at 50 At per cent.

Both the pressure and the temperature coefficients of the final system with C I are different from those of the virgin system. The temperature coefficients before and after subjecting to the pressure cycle have already been shown in Figure 11. The corresponding values for the pressure coefficients at atmospheric pressure are shown in Figure 17. The atmospheric pressure coefficients are also shown in Table IV. These coefficients were

TABLE IV

Initial Pressure Coefficients

Composition	Before 30,000	After 30,000
100 Bi	$+1.87 \times 10^{-5}$	
99.916	$+2.97$	$+2.01 \times 10^{-5}$
99.779	$+2.52$	$+1.05$
99.485	$+0.974$	-0.228
99.051	$\begin{cases} -0.136 \\ -0.473 \end{cases}$	-0.895 -0.965
98.25	-0.858	-0.923
97.98	-0.436	-0.843
95.ʊ	-0.705	-0.823
90.ʊ	-0.805	-0.835
89.88	-1.09	-0.880
80.ʊ	-0.938	-0.930
79.89	-1.03	-0.912
70.05	-1.094	-0.977
63.27	-1.096	-0.900
49.8	-1.057	-0.859
43.4	$\begin{cases} -0.975 \\ -1.034 \end{cases}$	-1.025 -0.907
36.2	-0.87	-0.822
29.72	-0.930	-0.950
19.99	-0.870	-0.88
10.08	-0.805	-0.872
5.07	-0.877	-0.821
2.05	-0.810	
100 Sn	-1.05	

found by calculating $\dfrac{\triangle R}{R_0} \cdot \dfrac{1}{p}$ at 5,000, 10,000 and 15,000 kg/cm²
and extrapolating to zero pressure. In the region of small tin con-
centrations there may be large changes in these coefficients, but
over most of the composition range the changes are comparatively
small, and usually in the direction of a numerical decrease of
coefficient after the pressure cycle. Table IV shows that the first
addition of a small amount of a second component at either end
of the composition range has the effect of numerically increasing
the pressure coefficient of resistance. As far as I know this rule is
without exception.

A few observations were made of the secular changes after a
pressure cycle. There are in the first place the observations already
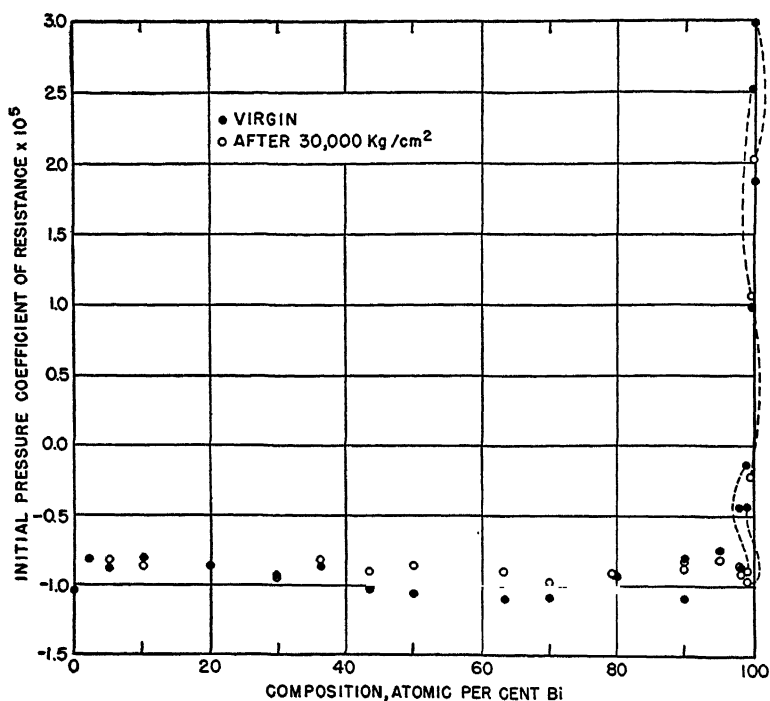
Figure 17. The initial pressure coefficient of resistance (that is, the co-
efficient at atmospheric pressure) of the bismuth-tin system as a function of
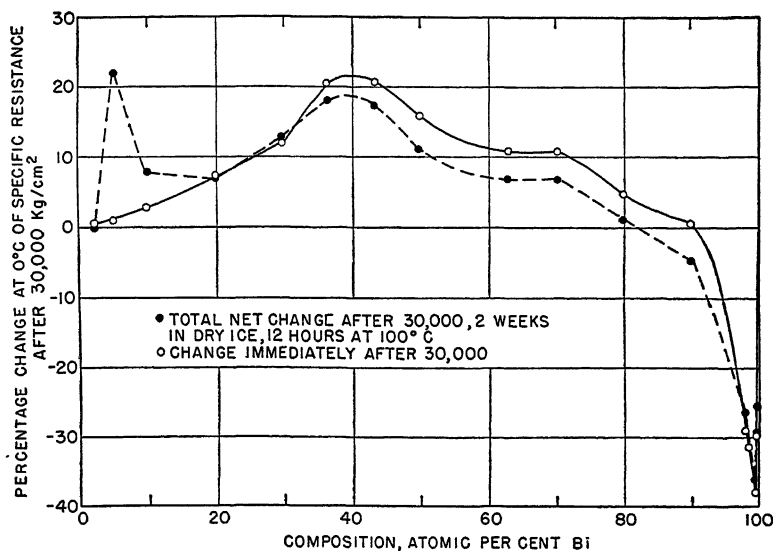composition, before and after exposure to pressure.

Figure 18. Secular changes of specific resistance in the bismuth-tin system after exposure to pressure.

referred to of the changes of resistance at 0°C after holding at dry ice temperature for two weeks and then at 100° for 12 hours. These results are shown in Figure 18. It will be seen that over most of the composition range exposure to 100° is followed by partial return to the resistance of the virgin specimen, but there are several notable exceptions, particularly at the tin rich end of the series.

The most extensive observations of secular change after 30,000 were made on the composition 70.05 Bi – 29.95 Sn. The resistance of this after the first exposure to pressure increased by 29 per cent, an unusually large amount, and quite out of line with the values shown in Figure 18. A second exposure to the 30,000 cycle was then made with negligible further change of resistance at atmospheric pressure. After the second pressure exposure the resistance at room temperature (fluctuations through 2°) was followed at atmospheric pressure for 450 hours. The drop of resistance was at first rapid, half the total observed change occurring in the first 20 hours. Above 150 hours the rate became practically constant, with no measurable difference in the rates at 150 and 450 hours. The total drop of resistance over the 450 hours was such as to

carry the specific resistance back to a figure 8.3 per cent greater than that of the virgin material.

Less extensive observations were made on eight other compositions. These will now be described, ordered according to composition.

97.98 At per cent Bi. Observed over 2.5 hours. After exposure to 30,000 resistance at atmospheric decreased by 27.48 per cent to 0.7252 virgin. In the first hour resistance dropped slightly further to 0.7251 virgin, after which the direction of motion reversed, leaving the resistance after 2.5 hours at 0.7255 virgin. The creep was almost within the errors of observation.

89.68 At per cent Bi. Observed for 17.5 hours. Atmospheric resistance after 30,000 was 4.3 per cent below virgin. With the lapse of time resistance continued moving in the *same* direction, and at the end of 17.5 hours was 6.0 per cent below virgin. The initial rate of decrease was 39 times faster than the final rate.

79.89 At per cent Bi. Observed for 20 hours. Atmospheric resistance after 30,000 was 26.6 per cent greater than virgin; after 20 hours this had decreased to 18.6 per cent greater. The initial rate of decrease was ten to twenty times greater than the final.

63.27 At per cent Bi. Observed for 3.8 hours. The atmospheric resistance after 30,000 was 18.7 per cent greater than virgin. After 3.8 hours this had decreased to 13.2 per cent greater. The rate was approximately constant over the entire time.

19.99 per cent Bi. Observed for 8 hours. Atmospheric resistance after 30,000 was 10.0 per cent greater than virgin. After 8 hours it had dropped to 8.0 per cent greater. The initial rate was approximately twice the average over the entire time, with an intermediate episode between 2 and 3 hours when the rate decreased below average followed by a rise to above average.

10.08 per cent Bi. Observed for 8.6 hours. Atmospheric resistance after 30,000 was 3.6 per cent greater than virgin. After 8.6 hours this had dropped to 2.6 per cent greater. The rate of drop was constant after 0.7 hour; the initial rate was approximately twice the average rate.

5.07 per cent Bi. Observed over 7.67 hours. Atmospheric resistance after 30,000 was 1.15 per cent greater than virgin. After 7.67 hours this had dropped to 0.74 per cent greater. The rate of drop was approximately constant over the entire time.

2.05 At per cent Bi. There was no change in atmospheric resistance after 30,000 and no creep during 3.5 hours, both to one part in 13,000, the limit of sensitiveness of the readings.

There is thus a wide variation in the character of these creep phenomena, with great variations in the absolute value of velocity, with inversions in the rate of fall and even reversals in the direction of creep (in two cases). It would therefore seem probable that no one simple type of mechanism would be adequate to explain the results. In particular, the rate of nucleus formation to the stable phase is not proportional to the amount of unstable phase from which the nuclei form.

E. *Electrical resistance to 100,000 kg/cm^2.* The experimental accuracy in measuring resistances to 100,000 is of a lower order than that in measuring to 30,000, as a consequence of the necessity of transmitting pressure with a plastic rather than a true liquid. There is a further special source of error with bismuth alloys in that correction cannot be applied so as to secure agreement with previous measurements in the interval 20,000 to 30,000 because of the occurrence of bismuth transitions in this interval.

The chief interest in making the present measurements to 100,000 was to find whether there are further internal changes in addition to the formation of the compound BiSn or whether any of the high pressure bismuth transitions above 40,000 kg/cm^2 are accompanied in the alloys by measurable changes of resistance. It is known that the volume discontinuities of the pure bismuth transitions above 30,000 are not accompanied by measurable discontinuities of resistance.

The technique of the measurements was the same as in the former published measurements and the same apparatus was used. At first the specimens were prepared by rolling the extruded wire flat. This method of preparation produced visible imperfections and cracks and gave irregular measurements of resistance. These difficulties were avoided by squeezing the extruded wire flat between polished carboloy platens in an arbor press. The final thickness was of the order of 0.002 inch, squeezed from wire 0.020 inch in diameter.

In Figure 19 is shown the ratio of the resistance at 65,000 kg/cm^2 to that at 30,000, and also the ratio of resistance at 100,000 to that at 30,000. The ratios shown were given directly by the
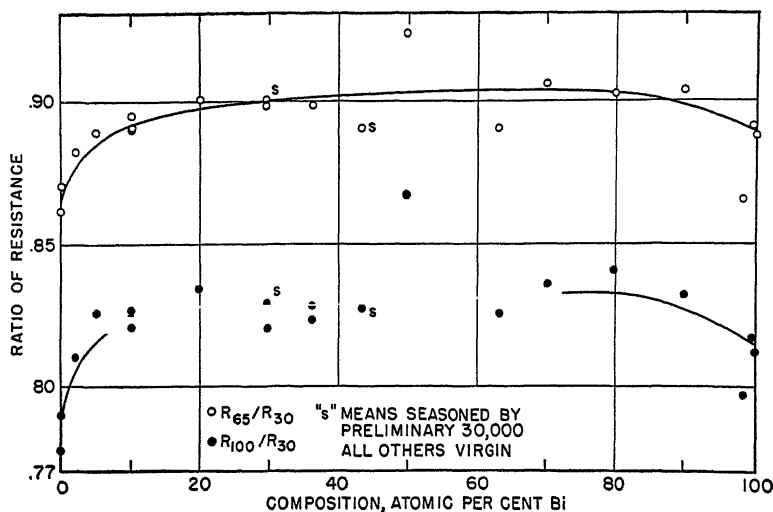
Figure 19. Summary of the resistance measurements on the bismuth-tin system to 100,000 kg/cm². The upper curve shows the ratio of resistance at 65,000 kg/cm² to the resistance at 30,000, and the lower curve the corresponding ratio for a pressure of 100,000, both as a function of composition.

readings without correction of any sort. There is considerable scatter, but no more than is characteristic of the method. In previous work four specimens of each material were measured to minimize the effect of scatter. Here, measurements were repeated only occasionally. The large number of specimens varying through a range of compositions permits an equivalent smoothing of results.

It would seem that there are no new episodes in the region above 30,000 large enough to be reflected in the behavior of resistance within the error of the method. The behavior of resistance of all compositions is normal in that it decreases with diminishing slope with increasing pressure. The arithmetical values of the pressure coefficients seem to tend to be smaller in the middle of the composition range, and the curvature of the relation between resistance and pressure greatest at the two ends of the range.

Comparison with previous results for the pure metals makes it probable that both the curves of Figure 19 are somewhat too high over their entire course.

Shearing Under Pressure

The measurements were made with a new carboloy apparatus to be described in another place. In principle it is similar to the apparatus of steel with which extensive measurements have been made.[6] With this new carboloy apparatus pressures of 100,000 kg/cm^2 are attainable as against 50,000 with the steel apparatus. In the present investigation pressure was pushed to the full 100,000 only a few times, and was usually restricted to 80,000 in the interest of longer life of the apparatus, the refiguring after use being much more exacting with carboloy than with steel.

Since the principal interest of these shearing measurements is qualitative, measurements were made on only a few compositions, namely: 100, 97.97, 79.89, 63.27, 43.4, 19.99, 10.08 At per cent Bi and 100 per cent Sn. All of the curves of shearing stress against pressure, except that for pure tin, showed a low pressure episode in the general neighborhood of 25,000 kg/cm^2, corresponding to the known bismuth transition here and to the irreversible formation of BiSn II near 20,000. The episode in all cases consists of a point of inflection followed by an upturn of the curve with eventual more rapid rise above 25,000 than at lower pressure. On the bismuth rich end the episode is more accentuated, consisting of a rise to a maximum, drop to a minimum, followed by accelerated rise. With diminishing bismuth content the maximum and minimum smear into each other and disappear, at the same time that the mean pressure of the episode drops approximately 5,000 kg/cm^2. All the curves except that of pure tin show a second episode in the general neighborhood of 65,000 kg/cm^2. Above this pressure shearing strength universally rises with pressure more rapidly than below. The episode is usually approached from below 65,000 by a decrease in the rate of rise and a point of inflection, although in one or two cases the preliminary decrease tends to disappear and the episode takes on more the character of a cusp. Over the middle of the composition range there is not much change in the episode, whether the composition is on the bismuth side, or whether the bismuth is entirely contained in the BiSn II phase. Bismuth is known to have a transition at 65,000; it would appear therefore that BiSn II also has a transition near the same point. The transition in pure bismuth is not accompanied by a

change in specific resistance; it would appear that the change of resistance at the BiSn transition is also small.

With regard to the absolute values of flow strength, they are small through the series, and rise from approximately equal values for pure tin and pure bismuth to a flat maximum at intermediate compositions. The value under 100,000 kg/cm² pressure is approximately 3,300 kg/cm² for the pure components, rising to 4,200 at intermediate compositions.

X-Ray Analysis

Professor Clifford Frondel of the Mineralogical Department of Harvard University most kindly undertook an X-ray examination in the effort to find the nature of the permanent change produced after exposure to high pressure, that is, to find the nature of C I. For this purpose material of 43.4 At per cent Bi – 56.6 At per cent Sn was rolled to a thickness of 0.0015 inch. One piece was exposed to 30,000 kg/cm² for approximately 15 minutes, then held at 18,500 for 20 minutes, and then pressure released to atmospheric in 5 minutes, the specimen immediately cooled to dry ice temperature and within another 5 minutes mounted in the X-ray camera where it was maintained below 0°C during the exposure of several hours. Professor Frondel describes the results as follows. The " treated " foils were those exposed to pressure as above, the " untreated " foils the controls from the same rolling.

" The treated foils appear to differ structurally from the untreated foils. I do not feel, however, that the available evidence establishes this fact conclusively. The evidence so far as it goes would be in line with a theory that the untreated foil had a disordered structure and that your treatment brought about an ordering.

" The X-ray photographs were taken in filtered copper radiation by transmission perpendicular to the surface of the foil, using the flat film method. The foils were bathed during the exposure in a stream of CO_2 from dry ice held in a funnel immediately above the sample; the sample temperature was below 0°C as shown by the development of ice at the edges of the foil (no ice was present on the area being X-rayed). Five photographs were taken: two of the untreated foils, two of the treated foils, and one of a treated foil that had been warmed almost to melting.

" The treated foils gave grainy patterns that indicated a considerable coarsening of the crystal size of the metal. The warmed foil gave a very grainy pattern, indicating a further recrystallization. The patterns of the treated and of the warmed material were identical, and they differed from that of the untreated foil in the presence of extra lines. I have not been able to determine the lattice type, symmetry or unit cell dimensions from the patterns. There were marked absorption effects on all the films."

The general conclusion would seem to be that there is not much difference between the lattices of C I and of bismuth, a conclusion in line with the volume relations. An extension of the same argument would suggest that the lattices of Bi II and C II are also similar. This means that some of the bismuth sites in the bismuth lattice can be occupied by tin without destroying the capacity of the lattice to undergo its polymorphic transitions. Under normal conditions the average atomic radius of bismuth is 9.3 per cent greater than that of tin. One would anticipate that pressure would favor the formation of a lattice of atoms of such different size.

THE BISMUTH-CADMIUM SERIES

Bismuth and cadmium are listed in works on metallurgy[7] as being completely immiscible in all relative concentrations, so that the pressure phenomena may be expected to be especially simple. In order to check the immiscibility, an alloy was made of approximately 2 At per cent of cadmium in bismuth and the resistance measured to 30,000 kg/cm^2. The resistance of this alloy was not far from that of pure bismuth, and the effect of pressure on resistance not dissimilar to the effect of pressure on the resistance of pure bismuth. There was no suggestion of the anomalous behavior shown by dilute tin in bismuth. There seemed no reason therefore to question the immiscibility of the two components, and further investigation was confined to three alloys, of approximately 25, 50, and 75 At per cent. The same sort of measurements were made on these as on the bismuth-tin series, except that since no new phases were formed, many of the former phenomena did not occur, such as permanent changes after application of pressure or creep back toward virgin values.

The alloys were prepared in the same manner as before. The

same bismuth was used, and "spectroscopically pure" cadmium from the New Jersey Zinc Company.

Density. The density at room temperature, approximately 25 °C, is shown in Table V and Figure 20. The variation with composition gives no suggestion of solution or compound formation, and is the sort of thing to be expected. The density at the mid point is what would be calculated assuming additive volumes.

TABLE V

DENSITY OF BI-CD SERIES

Composition At % Bi	Density
100.00	9.788
75.58	9.616
49.95	9.361
24.90	9.038
0.00	8.648

The density of this series has also been measured by Gabe and Evans.[8] Their densities are consistently somewhat higher than mine; the discrepancy rises to a maximum of 0.04 on the density at the middle of the composition range.
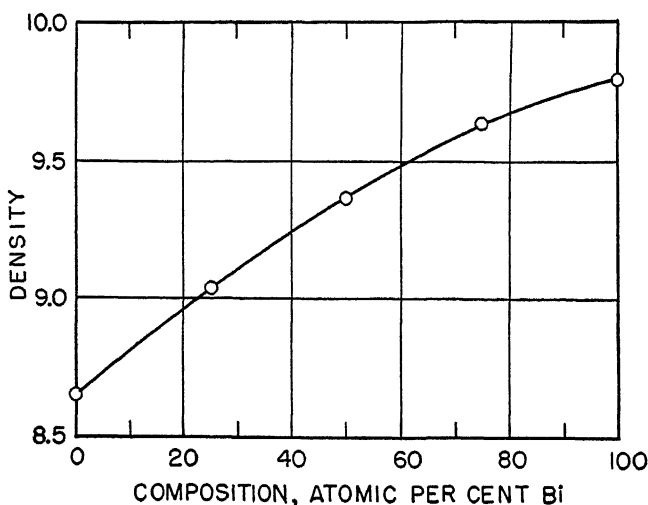
Figure 20. The density at atmospheric pressure and room temperature of the bismuth-cadmium system as a function of composition.

Compressions to 40,000 kg/cm². Measurements were made by the same method as for the Bi-Sn series. Again the alloys were sufficiently soft so that it was not necessary to use an indium sheath. Because there were no other transitions to complicate the picture it was possible to resolve the I-II and II-III transitions. In Table VI are given the relative volume compressions as a func-

TABLE VI

$\Delta V/V_o$ TO 40,000 kg/cm² OF Bi-Cd SERIES

Pressure kg/cm²	100.00 Cd	75.10 Cd 24.90 Bi	Composition, At % 50.05 Cd 49.95 Bi	24.42 Cd 75.58 Bi	100.00 Bi
5,000	0.0100	0.0124	0.0132	0.0139	0.0163
10,000	.0194	.0234	.0250	.0262	.0290
15,000	.0283	.0337	.0354	.0377	.0410
20,000	.0368	.0430	.0459	.0485	.0520
25,000	.0449	⎰.0523	⎰.0552	⎰.0586	.0624
		⎱.0693	⎱.0846	⎱.0987	
28,000		⎰.0748	⎰.0897	⎰.1034	
		⎱.0902	⎱.1115	⎱.1322	
30,000		.0934	.1149	.1355	.1526
35,000		.1005	.1226	.1429	.1601
40,000		.1067	.1291	.1494	.1670

tion of composition and pressure; these values are plotted in Figure 21. The data for pure cadmium were taken from previous work[9] and run to only 25,000 kg/cm² instead of the full 40,000. At constant pressure compression varies smoothly with composition, again giving no indication of any transition or solution phenomena. This holds for the high pressure modifications as well as for the low pressure modifications. Although smooth, the compression at a fixed pressure is not a linear function of composition within limits of error. The proportional deviation from linearity tends to be greater at the lower pressures.

Electrical resistance. The same measurements were made as for the bismuth-tin series: specific resistance at atmospheric pressure, mean temperature coefficient at atmospheric pressure between room temperature and 0°C, and resistance as a function of pressure at room temperature both to 30,000 and to 100,000 kg/cm². Wires were prepared for the resistance measurements by extrusion at about 150°C to a diameter of 0.020 inch. For this the other half of the slugs that had provided the compression specimens was used.
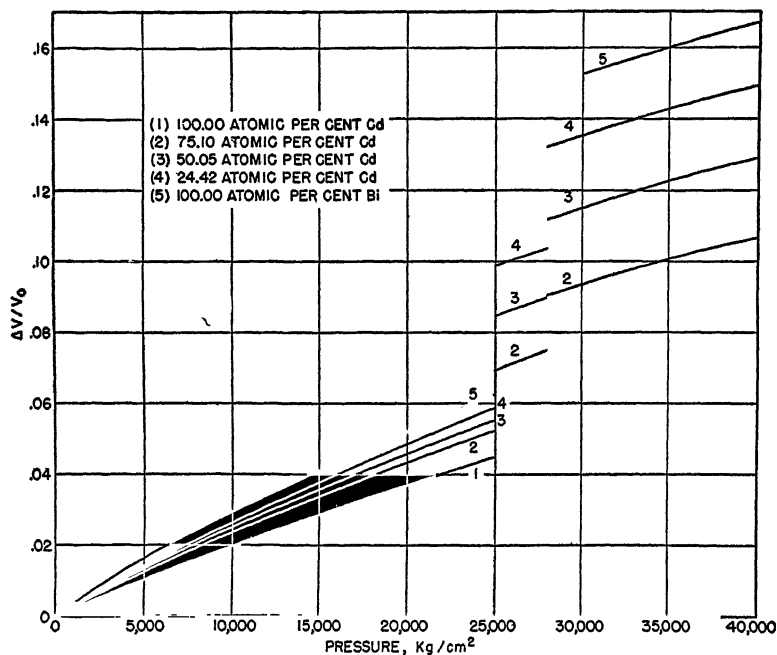
Figure 21. The volume compressions as a function of composition of the bismuth-cadmium system. Between 25,000 and 27,000 kg/cm² the system contains the phase Bi II, and above 27,000 the phase Bi III. The breaks in the curves correspond to the bismuth transitions.

A. *Resistance at atmospheric pressure.* The specific resistances and temperature coefficients are shown in Table VII. The specific resistance of 98 per cent bismuth is greater than the resistance of pure bismuth by a not large but nevertheless unmistakable amount, doubtless the result of lattice imperfections introduced by the cadmium impurity. Except for this, specific resistance rises smoothly from cadmium to bismuth, the rate of rise becoming rapidly greater near the bismuth end. The temperature coefficient of resistance shows no such extreme variations as in the bismuth-tin series, where there were reversals of sign, but drops at intermediate compositions below the extreme values. The drop is greatest at the bismuth end.

B. *Resistance to 30,000 kg/cm².* The "specific resistances" as a function of pressure at 0°, reduced from room temperature, for

TABLE VII

RESISTANCE DATA AT ATMOSPHERIC PRESSURE FOR
BI-CD SERIES AT 0° C

Composition At %	Specific Resistance	Temperature Coefficient of Resistance at 0° C
100.00 Bi	$109. \times 10^{-6}$	.00383
98.05	117.7	.00293
75.58	44.80	.00286
49.95	21.89	.00340
24.90	11.15	.00398
100.00 Cd	6.4	.00401

TABLE VIII

EFFECT OF PRESSURE TO 30,000 kg/cm² ON "SPECIFIC RESISTANCE"
OF BI-CD SERIES AT 0° C

Pressure kg/cm²	100% Bi	98.05% Bi	"Specific Resistance" $\times 10^6$ 75.58% Bi	49.95% Bi	24.90% Bi	100% Cd
0	109.00	117.7	44.800	21.89	11.150	6.400
5,000	119.78	130.4	44.997	21.41	10.734	6.147
10,000	131.94	144.0	45.149	20.97	10.370	5.931
15,000	145.63	160.3	45.248	20.59	10.059	5.743
20,000	160.66	175.2	45.194	20.26	9.790	5.571
25,000	178.66	192.3	44.839	19.96	9.535	5.432
	23.9 ±	24.80	17.38	11.42	7.979	
27,000	23.7 ±	24.54	17.07	11.34	7.813	
	98.2	63.32	28.50	15.17	8.742	
30,000		62.80	28.46	14.94	8.599	5.302

different compositions are shown in Table VIII. The "specific resistance" shown in this table is the ordinary specific resistance at atmospheric pressure multiplied at higher pressures by R_p/R_o. That is, it is the true specific resistance uncorrected for change of dimensions under pressure. The column for pure cadmium was taken from previous measurements,[10] assuming the same fractional changes at 0° as at room temperature. Again it was possible to resolve the bismuth transitions, and values are shown at 25,000 and 27,000 for the Bi II phase, and at 27,000 and 30,000 for the Bi III phase. From these figures it is possible to find the pressure coefficients of the high pressure phases, but these have relatively low accuracy because of the short pressure range and involvement with other phases. In particular, no significance should be attached to

the relative changes with composition of the pressure coefficients of the high modifications that would be calculated from the table.

The resistances of pure bismuth and the bismuth rich end of the series in the low pressure modification are not single valued in pressure, but resistance with increasing pressure on the initial application of pressure is not the same as the resistance with decreasing pressure on the final release after the transitions have occurred. The reason for this, as already indicated, is that the original orientation is not recovered after occurrence of the transitions, the low pressure form being markedly anisotropic. Since there is no definite indication of what the orientation or its change is, the best that can be done is to average resistances with increasing and decreasing pressure on the low pressure phase. There is no such uncertainty with respect to the high pressure phases, but resistances for them are single valued, except for uncertainties sometimes arising from incomplete running of a transition.

Qualitatively, the resistance of the low pressure phase behaves as would be expected. The resistance of the approximately 2 per cent Cd composition increases with pressure, and at first with increasing curvature, as does also that of pure bismuth. This curvature becomes less at higher pressures, however, and shows signs of reversing at the upper end of the pressure range. On release of pressure, resistances were a few per cent higher than on increasing pressure, with a definite tendency to drift by a small amount after every decrease of pressure. This direction of drift is downward, and is perceptible as far down as 10,000 kg/cm². It is in the opposite direction from the drift to be expected from either incomplete running of the transitions from II to I or from recovery of temperature equilibrium after the adiabatic pressure change. The only explanation which presents itself for this small drift is plastic deformations in the grains due to non-isotropic changes of dimensions after changes of pressure; there is no apparent good reason for drift down rather than drift up.

The final zero of resistance of the 75.58 per cent specimen was 6 per cent lower than that of the virgin specimen. With increasing pressure resistance was approximately constant for the first 10,000 kg/cm², followed by a decrease with increasing downward curvature. On decreasing pressure the curvature is also downward. There was a maximum resistance in the neighborhood of 20,000

followed by a decrease of approximately 2 per cent. Increasing and decreasing readings were averaged for the table.

The resistance of the 49.94 per cent specimen decreased with normal upward curvature for both increasing and decreasing pressure. The resistance with decreasing pressure was from 1.5 to 2.0 per cent lower than with increasing pressure.

The resistance of the 24.90 per cent specimen was normal in every way, the resistance dropping with upward curvature with increasing pressure and with no perceptible difference between up and down readings.

There was no evidence of a displacement of the equilibrium pressure between the high pressure modifications with changing composition. No special effort was made to establish this with maximum possible precision. This would have been increasingly difficult with increasing cadmium content because the sharpness of the transition tends to get smeared out, with progressively increasing hysteresis and widening of the pressure limits within which the transition will start with increasing and decreasing pressure. The transition from II to I with the 24.90 per cent Bi composition did not start until a pressure of about 23,000 was reached with decreasing pressure, against an equilibrium pressure of 25,000.

The ratios of the resistances when the high pressure transitions run do not vary symmetrically over the composition range. Both of these ratios of necessity drop to the limiting value unity for pure cadmium. The ratio R_I/R_{II} drops by a considerably larger factor over the composition range than does the ratio R_{II}/R_{III}, and the variation is more localized at the bismuth rich end. The former drops continuously with increasing cadmium content, whereas the latter rises to a maximum with 1.95 At per cent Cd content. The resistance of both high pressure forms is so much higher than that of pure cadmium that the net resistance of the whole system drops markedly with increasing cadmium content in the high pressure regions as well as in the region of low pressure. There was nothing here similar to the behavior of the bismuth-tin system in which one of the high pressure phases had a resistance nearly independent of composition over the entire range.

C. *Resistance to 100,000 kg/cm²*. Measurements were made in the regular way on the three intermediate compositions. In ad-

dition, pure bismuth was specially measured again in connection with the bismuth-tin series and there are also values for both pure cadmium and pure bismuth from previously published work.[11] The specimens were prepared by squeezing flat between carboloy platens the extruded wire used for the resistance measurements to 30,000. The specimens showed no sign of mechanical imperfections.

Again, as in the case of the bismuth-tin series, it was not possible to correct the high pressure values so as to secure agreement with former values in the 20,000-30,000 range, but the results have to be taken at their face value.

The resistance of all specimens varied smoothly over the pressure range, with no evidence of further high pressure transitions. It would thus appear that there is no mutual solubility or compound formation in the bismuth-cadmium series, either with the usual low pressure modification of bismuth or with any of the five high pressure modifications.
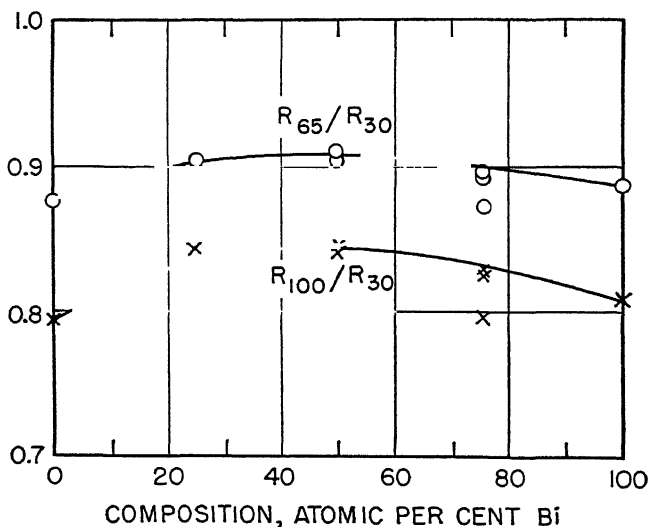
Figure 22. Summary of the resistance measurements on the bismuth-cadmium system to 100,000 kg/cm². The upper curve shows the ratio of resistance at 65,000 kg/cm² to resistance at 30,000, and the lower curve the corresponding ratio for 100,000, both as a function of composition.

The ratio of resistance at 65,000 and at 100,000 kg/cm² to the resistance at 30,000 was calculated in the same way as for the bismuth-tin series, and is shown with sufficient accuracy in Figure 22. Both ratios rise in the middle of the composition range compared with the pure components. This means a smaller absolute value for the pressure coefficient of the intermediate compositions. There is also a greater deviation from linearity in the relation between resistance and pressure in the middle of the composition range.

Shearing under pressure. Measurements were made in the regular carboloy apparatus on the three intermediate compositions. The pressures were carried only to 80,000 instead of the full 100,000. All three compositions showed the expected incident accompanying the formation of the high pressure forms in the neighborhood of 25,000, diminishing in magnitude toward the cadmium end, namely diminished rate of rise of shearing strength, with point of inflection followed by more rapid rise. For the 75 and 50 At per cent bismuth compositions the rise above 25,000 continued to accelerate upwards up to the maximum pressure, but for the 25 per cent composition the curvature eventually reversed to concave downward, normal behavior for most metals. On releasing pressure on the 75 per cent specimen there was a second anomaly in the neighborhood of 45,000, presumably due to the known bismuth transition at 45,000. This is the first manifestation of this transition found in any of the measurements of this paper.

With increasing cadmium content there is at first an increase of absolute shearing strength beyond that of pure bismuth, followed by a continuous decrease. The absolute shearing strengths were, for the compositions 75, 50 and 25 At per cent Bi, 7,800, 6,600, and 4,700 kg/cm² respectively at the top pressure. These figures are materially higher than for the bismuth-tin series.

SUMMARY

The following measurements have been made on three compositions (five, counting the pure components) of the bismuth-cadmium series and approximately twenty of the bismuth-tin series: at atmospheric pressure: density, specific resistance and temperature coefficient of resistance; under pressure at room tem-

perature: volume compressions to 40,000 kg/cm², resistance to 30,000 kg/cm² under truly hydrostatic pressure and to 100,000 kg/cm² under approximately hydrostatic pressure, and occasional measurements of plastic shearing strength to 100,000 kg/cm².

The bismuth-cadmium system under normal conditions is a simple eutectic system with no appreciable solubility of either component in the other. This remains the situation under pressure, so that the properties are simple and approximately what would be expected from a mechanical mixture of the pure components. In particular the two transitions of bismuth at approximately 25,000 and 27,000 kg/cm² continue to occur independent of the presence of the other component and with no displacement of the transition pressures.

The bismuth-tin system is also under normal conditions a simple eutectic system but with mutual solubility of the two components. Under pressure, complications occur in the bismuth rich end of limited tin solubility, but no appreciable complications appear at the tin rich end, where the pressure phenomena would not suggest solubility at all. The principal complications at the bismuth rich end are connected with electrical resistance. The specific resistance increases by a factor of nearly sixfold at one per cent atomic tin. The " additive " resistance due to tin is very pressure sensitive, decreasing under pressure, whereas the resistance of pure bismuth increases. However, for very small amounts of added tin the universal rule is obeyed that the addition of a foreign component increases the pressure coefficient of resistance algebraically. The maximum pressure coefficient occurs at about one tenth atomic per cent tin. In the region of a few atomic per cent tin the total pressure effects are highly complicated, with reversals of curvature, maxima, and reversals of sign.

Beyond a few atomic per cent tin a new complication appears in the bismuth-tin system. An alloy subjected to a pressure of 25,000 kg/cm² or more experiences an irreversible change which persists on release of pressure to atmospheric with slow reversion. A study of the way in which these changes vary with composition indicates that the change consists in the formation of the compound BiSn. This compound itself has a high and a low pressure form which change into each other approximately reversibly but with some hysteresis in the range between 15,000 and 20,000 kg/cm².

The initial irreversible formation of the compound from the virgin eutectic mixture occurs only to the high pressure form and therefore only above 20,000 kg/cm². The crystal structure of the irreversibly formed product at atmospheric pressure is not known, an X-ray analysis showing only minor changes in the pattern. The volume relations are such as to indicate that in the compound the bismuth atom occupies approximately the same volume that it does in pure bismuth in that polymorphic form corresponding to the external pressure. That is, at low pressures the total volume of the system may be approximately calculated by assigning to the bismuth atom, whether in the compound or in the unaltered eutectic, the same volume that it occupies in the low pressure form of pure bismuth, and at high pressures, above 27,000 kg/cm², the total volume of the system at any composition may be approximately calculated by assigning to the bismuth atom, whether in compound or eutectic, a volume equal to the volume of the bismuth atom in pure Bi III. With regard to electrical resistance, there is a striking approximate constancy of specific resistance of the whole system, independent of composition, in the pressure range of stable existence of Bi II, this in the face of a twentyfold change in the contribution to resistance made by the Bi I-Bi II transition. The pressure of the bismuth transitions was not affected by the presence of tin. No phenomena were observed suggesting a change of solubility limits with pressure.

I am much indebted to Mr. Charles Chase for assistance in these experiments.

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Temperatures below
one degree Kelvin

C. J. GORTER

Temperatures below

one degree Kelvin

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Temperatures below 1° K [*]

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Historically the notion of temperature has two very different theoretical backgrounds. One of these backgrounds, connected with the kinetic theory of gases, as it was developed in the 19th century by *Clausius* and *Maxwell*, has to do with the random motions of molecules and atoms. According to its central statement the absolute or Kelvin temperature is proportional to the average kinetic energy of free molecules. This statement is obviously not applicable if we admit interactions between the molecules, such as exist in a real gas or, much more intimately, in a liquid or a solid. What then remains is a somewhat more complicated connection between the probability for a system of particles to find itself in a particular state of energy and the Kelvin temperature, a connection given by the so-called *Boltzmann* distribution factor which tells us that on the average low energy states are occupied more frequently than high energy states, but that the higher energies occur more frequently the higher the temperature is.

The second background, created somewhat earlier in the 19th century by *Carnot* and *Kelvin*, has an industrial origin. It has to do with the maximum efficiency of engines or motors which transform heat into mechanical energy as well as of refrigerating machines. These engines, motors and refrigerating machines operate between a low temperature T_l and a high temperature T_h. The engines and motors take up the heat Q_h at the higher temperature T_h and give off a smaller amount of heat Q_l at the lower temperature T_l while the refrigerating machines, on the contrary, absorb heat at the lower temperature, giving off a larger amount at the higher temperature. In both cases the difference is made up by potential energy or work, delivered in the first case and consumed in the case of the refrigerating machine. Now, according to *Carnot* and *Kelvin*, the cyclic process has its maximum efficiency if the ratio of the two temperatures and the two amounts of heat

is equal: $T_h/T_l = Q_h/Q_l$. In fact this maximum efficiency is reached when all frictional losses are avoided.

It has been the achievement of *Boltzmann*, *Gibbs* and *Ehrenfest* in the end of the 19th and in the beginning of the 20th century, to show that the kinetic and the thermodynamical backgrounds are in complete agreement with each other as long as usually insignificant fluctuation processes are neglected. Thanks to them we may now use the two backgrounds and their mathematical formalisms alternatingly and at will without being afraid of internal contradictions.

It is one of the most essential qualities of temperature that, in a system which is left to itself, it tends to become uniform. Thus a thermometer placed in a bath accepts soon its temperature.

Historically the drive to ever lower and lower temperatures was realized in steps, and even in the most modern engines to obtain very low temperatures these, or similar, steps are repeated. Starting from the housewives' temperature interval between 273°K and 373°K, the first step is to the liquid air region between 60° to 90°K; next comes the liquid hydrogen interval from 14° to 20°K and the liquid helium region between 4.2°K and about 1.0°K and finally the region of tonight's subject: that obtainable by adiabatic demagnetization from 1°K to perhaps 0.001°K or further downwards. Seen on the usual linear temperature scale, the steps become smaller and smaller and the achievement may seem of less and less importance. However, the fact that in the thermo-dynamical background we have essentially to do with ratios of absolute temperatures instead of with differences, just as sometimes occurs in the physiological response on excitation, encourages us to introduce a logarithmic temperature scale in analogy to the well known decibel system of acoustics. It is clear that such a scale, according to which the liquid helium interval and the range attainable by adiabatic demagnetization easily outweigh the more usual temperature ranges, appeals to the vanity of the low temperature physicist, but he has to admit that this logarithmic scale, which suggests that a temperature interval between 0.1 and 0.01°K is as important as that between 100° and 1000°K, is only fair and adequate in the study of certain specific properties of matter. In accordance with *Nernst's* law many properties of matter, among which I mention the density, the

heat of vaporization, the elastic and dielectric properties, do not change materially at very low temperatures. But the electrical conductivity, the heat conductivity and paramagnetism do. Of those the paramagnetic properties are of prime importance for the low temperature physicist since they have to do with essentially reversible phenomena, and thus may be used in refrigerating processes.

The mechanisms used in the first steps of the downward drive are of two different natures. They either aim directly at a decrease of the kinetic energy of random motions inside a system or at an increase of its potential energy at the expense of the kinetic energy.

In order to make the mechanisms of these processes somewhat more familiar to the non-physicists in the present audience, let us compare the kinetic energy of particles to cash money belonging to individuals and their potential energy to their bank accounts. The aim of the low temperature physicist is to reduce the cash and he can do this either directly, by taxing and other means, or by persuading the individuals to increase their bank accounts at the expense of their available cash. When a gas expands, while doing work, the molecules strike a backward moving piston, coming back at reduced velocity, part of their kinetic energy thus having been taxed away. If the temperature on the contrary is reduced by evaporating a liquid adiabatically, as in the usual household refrigerators, or by expanding a gas through a nozzle, the kinetic energy of the molecules decreases because of their increasing potential energy. In order to obtain a repeatable closed cycle it will be necessary to compress the gas or to condense the vapour beforehand or afterwards, while leading away the heat that is then developed. The decrease in cash is caused at the moment when the individuals are persuaded to increase their bank accounts. If one wishes to obtain a continuously working cyclic process it will be necessary to have the cash money spent when, in another phase of the cycle, it is plentiful. The magnetic process invented by *Debye* and *Giauque* to produce cold at temperatures under $1°K$ is analogous to the last mentioned methods of producing cold by evaporating a liquid. The required increase of potential energy, however, is not obtained by an increase of distance between the individual molecules but rather by an increase of the

magnetic energy connected with the presence of an electro-magnet. So far, because of experimental difficulties, very few attempts have been made to work in a repeatable closed cycle and the usual procedure to reduce the kinetic energy consists of only one decrease and one subsequent increase of the potential energy, or the bank accounts if you wish. Thus first the permitted bank accounts are made to differ so considerably that nearly all indi-viduals have to choose the lower one of the accounts. Then it is necessary to wait till the cash, that has been paid out, is largely spent. Subsequently the differences between the permitted bank accounts are reduced drastically, thus luring the individuals to place their scarce remaining cash in the bank. The magnetic process thus consists of first subjecting a paramagnetic substance such as chromium or iron alum, containing many magnetic ions, to a high magnetic field. The large majority of the ionic magnets then place themselves parallel with the field, thus reducing their potential energy and developing heat which is carried away to a bath of liquid helium which is in thermal contact with the sub-stance. Then the substance is thermally insulated from the helium bath and the magnetic field switched off. During this adiabatic demagnetization, the elementary magnets then try to reassume their original random orientations by reducing all other energies available and thus cooling the substance down.

The next problem which then faces the low temperature physi-cist is the determination of the very low temperature thus obtained in his sample. This is carried out by performing a thermodynamic cyclic process with the sample and applying the central relation of our second background: $T_h/T_l = Q_h/Q_l$. After reaching the temperature T_l by adiabatic demagnetization a small amount of heat Q_l is introduced. Then the substance is remagnetized up to the original field. The temperature is then slightly higher than the original one and by extracting the amount of heat Q_h that original temperature is restored and the cycle thus closed. If our higher temperature T_h, which is in the neighborhood of $1°K$, is known and the Q_h and Q_l are measured, we can thus calculate the unknown lower temperature T_l. The chief experimental prob-lem is how the amount of heat Q_l is to be applied and measured. In Oxford this is done by irradiation with gamma rays and in Leiden by magnetic methods.

The magnetic properties of the cooling substance may be used as provisional thermometers and, by carrying out cyclic processes as sketched, we may obtain a calibration of these provisional thermometers on the Kelvin scale. The lowest temperature measured so far was 1.4 millidegree Kelvin in a mixed crystalline sample of chromium and aluminium alum.

The first scientific result of the experiments is of course the determination of the magnetic properties of the cooling substance itself. It is found that at sharply defined temperatures of a few centidegrees or millidegrees Kelvin the salts of the iron group, which are used, undergo a so-called transformation of the second kind and become anti-ferromagnetic or perhaps ferromagnetic. The individual magnets then have lost their individual freedom and form more or less rigid patterns of an antiparallel or parallel character. They rapidly lose the possibility to react independently on outside magnetic fields and on small temperature variations and either do not react at all or only co-operatively with many millions of their fellow magnets. If I were not afraid that pursuing our metaphor would lead us into the fields of sociology or even politics, I could work out how lack of cash may entail the loss of the individual liberty to react on changes in banking policy and thus lead to a social structure of a very rigid character, which differs widely from the paramagnetic way of life.

It will also be possible to cool other substances down and study their properties at the very low temperatures reached, though the slow tempo in which heat, or rather cold, is transported and transmitted at temperatures under $0.1°K$ often presents a serious obstacle. A few new superconductors have been discovered in this temperature region and their curves of magnetic disturbance have been determined. Furthermore a few provisional investigations have been carried out on the heat conductivity of various substances and on specific heats.

A most fascinating group of problems is connected with Helium II, the unruly liquid, probably existing as such down to the lowest temperatures, with its perhaps just as intriguing liquid isotope ^{3}He and with mixtures of these two liquids.

Another very promising scientific application of very low temperature physics is the orientation or alignment of atomic nuclei, and it may be appropriate to devote some attention to that topic.

It has been mentioned that certain ions, such as chromic ions, cobalt ions, and iron ions are small magnets. Their magnetism is due to the spin motions and orbital motions of their electrons. It has been found that many atomic nuclei are small magnets too, their magnetism being due to motions of their constituents, the protons and the neutrons. These nuclear magnets, however, are about a thousand times weaker than the ionic magnets. This makes it relatively difficult to orientate them.

Looking for possibilities to extend our metaphor of the individuals with bank accounts, I contemplated comparing the nuclei with mice which would be able to have only very small bank accounts indeed. But this comparison between mice and men would possibly grieve the nuclear physicists without giving us the satisfaction of a clear-cut parallelism. And since it is not the low temperature man's purpose to hurt the nuclear physicist, but on the contrary to please and assist him, we rather drop the metaphor now.

The properties of nuclei must be different in different directions. The emission of radioactive radiations will in general not be equal in the direction of the nuclear magnet and perpendicular to it and the probability of reaction between two nuclei must strongly depend on the mutual orientation of the two nuclear magnets. The only simple way to study this requires the orientation of nuclei in a particular direction in space. Since nuclear magnets are about a thousand times weaker than ionic magnets, this would require a ratio of magnetic field to temperature which is a thousand times higher. Thus instead of, for instance, 5000 oersted and 1°K one would need 50,000 oersted and 0.01°K. At first sight this would not seem particularly difficult. First an adiabatic demagnetization of an ionic substance would lead to 0.01°K and then a sample, containing our nuclei, should be cooled down to that temperature in the presence of the large magnetic field. However, after the demagnetization the field would be zero or small and the nuclei would have to be placed in a very large magnetic field which, as was pointed out by *Simon*, would require transport of heat over a considerable distance, which transport seems hardly feasible at so low a temperature.

In 1948 *Rose* and we independently proposed a method to get around this difficulty for the nuclei of certain magnetic ions.

The electrons in these ions bring about a field of the order of a few hundred thousand oersted acting on their own nuclei. Thus after the demagnetization, the field acting on the ions could be zero or small, while nevertheless a very high field would act on the nuclei, being amply sufficient to orientate them. A careful analysis by *Bleaney* revealed that this could be most favorably realized in a single crystal and in the autumn of 1951 the first doubtlessly positive results on nuclear alignment could be reported from Oxford and from Leiden. The alignments were carried out with the radioactive nuclei of cobalt with mass number 60. After being aligned, these nuclei first emitted an electron, thus transforming themselves into nickel nuclei, and then went over into the ground state by consecutive emission of two gamma quanta. This gamma ray emission was considerably weaker in the direction of the magnet than perpendicular to it, which was an agreement with conclusions drawn by *Deutsch* from the angular correlation of the two gamma quanta. It was possible to evaluate the strength of the radioactive cobalt nucleus and attempts are being made to determine its sign too.

It will be possible to investigate a few more nuclei of paramagnetic ions, but for other nuclei one will have to rely upon extremely large external magnetic fields, combined with rather low temperatures. The installation of very powerful electromagnets in different low temperature institutes (another effective way to reduce available cash) should also be seen in this light.

It has been mentioned several times already that the transmission and the transport of energy become very bad at very low temperatures and this is because the heat waves, which have to do the transport, have almost died out.

I am afraid that our metaphor, in which the low temperature physicist was introduced as the man who does not hesitate to apply the most radical and unscrupulous methods in order to reduce the cash reserves of his clients and friends, has somewhat affected the esteem in which he was held by those who did not really know him and the sympathy to which he hoped to be entitled.

For sympathy is what he really wants since he is faced with the threat that his restless drive to lower temperature is soon going to come to its end. It is, in fact, already now apparent that the

greatly reduced thermal transmission and thermal conductivity permit the persistence of widely different temperatures between points a few millimeters apart in a single crystal. And unless relief is brought by a new heat conducting device applicable at temperatures of a few millidegrees Kelvin, his drive to always lower temperatures will very soon be checked, not because he cannot invent and apply refined methods to reduce the temperature, but because temperature is losing its essential quality of equalizing itself because the thermometer refuses to accept the temperature of the bath and one half of a sample can no longer be cooled by the other half. All he can do is to ascribe different temperatures not only to the different regions of his sample, but also to the different elements in the same region. He thus will have to distinguish between the temperature of his nuclear magnets, of his free electrons, of his ionic magnets and of his lattice heat waves and he has to confine himself to the investigation of the vanishing rates at which energy is exchanged between elements of different temperature.

But it is not yet quite so far. Many problems of low temperature physics, among which those of superconductivity and of Helium II, remain to be solved; several further applications may be invented and developed. And also the study of the rates at which energy may be exchanged between one element and the other may give plenty of work to at least one more generation of low temperature physicists.

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and the general will

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The Supreme Court
and the general will

ARTHUR E. SUTHERLAND

The Supreme Court and the general will*

Some Speculations on the Judicial Role in Tomorrow's Democracy

ARTHUR E. SUTHERLAND

Harvard University Law School

... la volonté générale est toujours droite et tend toujours à l'utilité publique: mais il ne s'ensuit pas que les délibérations du peuple aient toujours la même rectitude. On veut toujours son bien, mais on ne le voit pas toujours. ...

Il y a souvent bien de la différence entre la volonté de tous et la volonté générale; . . .[1]

I

The principal difficulty in government by majority is that the majority is sometimes wrong. Few men will always accept as a criterion of truth " the majority vote of that nation that could lick all others."[2] Instead, with comforting reliance on the power of reason, one postulates rightness, discoverable by taking thought; and deplores the occasional and, one hopes temporary, divergences therefrom by the multitude. The difficulty arises in distinguishing the occasions when the majority is wrong, from those instances in which the minority itself falls into error despite the superior discernment it is apt to sense in its members. Truth is exasperatingly elusive.

A nation devoted to the majoritarian principle but conscious of its proneness to occasional error, and hopeful that the swings of political power may, after a while, correct its mistakes, does well to look for some governmental device to define and correct majority error during the intervals of aberration. The requirement is an institution of unusual intelligence, detached from politics, and so respected that, at least for a while, a majority will follow it against the majority's own current of desires. In the United States

we have found such an institution in the Supreme Court. We have given it a limited mandate to correct mistakes made by state or national majorities, describing that mandate in conveniently general terms which permit the Court to adapt its judgments to unforeseen needs as they arise — defining its objectives by such phrases as " due process of law," and " freedom of speech or of the press," which express aspirations rather than rules. This duty it performs with some deference to legislative wisdom, often stated in terms of a " presumption of constitutionality."

With a mission described in such inclusive terms, with a duty to choose the good law and reject the bad, but with no convenient way of determining by an objective test which is which, the Court is sure from time to time to arouse substantial resentment. Of course, soon or late, by constitutional amendment or judicial interpretation, the majority will have its way. That law will be a good law which a majority desires with enough persistence. The role of the Supreme Court will, in the long run, be what the people want it to be. It is therefore important to see what the people of the United States from time to time expect of their Court.

This paper attempts only a cursory examination of one small segment of opinion during a limited time. Here I have only sought to refresh my recollection of what certain liberals thought of the Supreme Court's function a generation ago, and to see what has happened to that opinion since that day. And, lest I be thought too naïf in using the term " liberals," I make haste to acknowledge its unsatisfactory character. It means many things to many men; it varies in usage from generation to generation. One can only be confident in a negative — that it has not, in the time of men now in middle life, been generally taken to describe people who believed that government should keep hands off the economic processes of society.[3] There are terms that are obscured rather than clarified by attempts at definition. For the purposes of this discussion only, I take a " liberal " to mean the sort of man who enjoyed the *New Republic* about 1923.

II

At least one human trait is so common as to approach universality. It is a sort of intellectual myopia, tending to limit the view of each observer to that part of a problem directly under his

eyes. So those who have undertaken to appraise the part played by
our federal judiciary have sometimes been misled by undue atten-
tion to problems of the immediate moment. Thirty years ago,
liberal opinion tended to think of the Supreme Court only as an
obstacle in the way of desirable social and economic reforms. In
those simpler times, critics needed fewer doubts. Earnest young
writers knew, then, that the general will expressed itself through
freely elected legislatures; and that when the Supreme Court inter-
fered with the work of the legislators, the Supreme Court was
almost always wrong.

Some zealous critics knew that it was always wrong. Comments
of the practicing politician are apt to be less guarded than those
of the scholar, and the elder Senator LaFollette felt no need to
mince words when he spoke of the judicial power to find statutes
unconstitutional. In 1922 he told the American Federation of
Labor:

> From what source, it may be asked, have the Federal judges derived the
> supreme power which they now so boldly assert? Not only was such power
> not given to the judiciary in any Constitution, State or Federal, but the records
> of the Constitutional Convention show that when it was proposed . . . that
> judges should have a veto upon acts of Congress, it was decisively defeated
> on four separate occasions, and at no time received the support of more than
> three States. . . .
> There is, therefore, no sanction in the written Constitution of the United
> States for the power which the courts now assert. They have secured this
> power only by usurpation. . . .[4]

Academic commentators, somewhat more restrained, still
tended to view the Supreme Court with alarm. Francis B. Sayre,
then a professor at the Harvard Law School, wrote in the May,
1923, *Survey:*

> If the Fifth and Fourteenth Amendments are to be so interpreted that
> henceforth legislation is to be declared unconstitutional whenever it is out of
> accord with the economic and social theories of five members of the Supreme
> Court, a blow is struck at one of the most fundamental principles of our
> government. The legislature then becomes not an independent and supreme
> body framing policies into law; it becomes subordinate to the Supreme Court
> which becomes virtually a House of Lords, exercising an actual veto power
> of such laws as fail to accord with the social theories of five of its members.

Writing in the *Harvard Law Review* in 1924, Maurice Finkel-
stein, an eminent and philosophical member of the New York Bar,

urged that the Supreme Court renounce its jurisdiction of cases challenging social and industrial legislative policy for the same reasons which sometimes move it to forgo jurisdiction in cases where " political controversies " are involved.

[A] great deal of the so-called unconstitutional social and industrial legisla-tion might have fared better at the hands of the court had it been treated from the point of view of a political question. This would involve the sur-render on the part of the court of the prerogative of passing upon the policy of an act as enunciated by the legislature. . . . For, in truth, in cases involving the industrial policy of a state or nation the measure of due process approaches very closely to the measure of political wisdom. The function of determining the political policy of the government belongs to the legislature. . . . No matter in what terms the opinions of jurists have been couched, it is apparent that it is the fear of consequences or the lack of adequate data that has impelled the courts to refrain from entering upon the discussion of the merits of prickly issues. It seems to us that these very considerations should have compelled the refusal by the courts to take jurisdiction of cases raising the question of the constitutionality of social and industrial legislation. It is difficult for the courts, dependent as they are upon counsel for their facts, to have before them the material considerations that have caused state legislatures to pass a limited-hour day for laborers, or a minimum wage law for women and children. The legislative industrial policy of a state or nation can hardly even be stated in classical legal terminology. It would therefore seem the better statesmanship to have included all of these questions in the general category of " political questions ". . . .

The most interesting common feature of these and like com-ments of a generation ago is the concentration of attention on what was described as social and economic legislation; and the feeling that, left to themselves without court interference, legis-lators would do well for the people. To suggest that this attitude was anywhere near unanimous in the United States would, of course, be a distortion. But it could fairly be said to represent the " liberal " point of view. Mr. Finkelstein seemed untroubled by any possibility that a day might come when the Supreme Court's inaction in controversies which it called " political " would pro-duce liberal protest. He did not foresee the Court's refusal to require a correction of gross inequality of congressional districting in Illinois,[5] or of the comparable " county unit rule " in Georgia.[6] His recognition of the political importance of much industrial and social legislation was fully justified. But he apparently foresaw no possible application of this doctrine of judicial abstention to suits

brought before the federal judiciary seeking to correct state in-justice to the Negro — a political issue of some warmth. That " human rights " are not always easy to distinguish from " property rights "; that judicial restraint in cases of the latter sort, but not in the former, would be difficult to achieve — this seemed to cause little worry in the mid-twenties.

American liberal opinion tended to approve of the Congress and to regard assertion of constitutional rights as legalistic and in-sincere. Witness the reaction to a presidential reminder.

On April 11, 1924, Mr. Coolidge sent a message to the Senate protesting at the manner of the conduct of Committee investiga-tion of a Cabinet officer — the Secretary of the Treasury, Andrew Mellon. The President wrote:

> Under a procedure of this kind, the constitutional guarantees against unwarranted search and seizure break down, the prohibition against what amounts to a Government charge of criminal action without the formal presentment of a Grand Jury is evaded, the rules of evidence which have been adopted for the protection of the innocent are ignored, the department becomes the victim of vague, unformulated and indefinite charges, and instead of a Government of law we have a Government of lawlessness.[7]

With editorial scorn the *New Republic* of April 23rd took the President to task:

> ... Mr. Coolidge accuses the Senate of lawlessness [in] the evasion by com-mittees of that body of "the prohibition of what amounts to a government charge of criminal action without the formal presentment of a Grand Jury," and the ignoring of "the rules of evidence which have been adopted for the protection of the innocent." ... It is based on the preposterous assumption that an investigation into the conduct of public officials who are suspected of wrong doing should be conducted under all the limitations of a criminal trial. If the Walsh and Wheeler committees had not acted in the way which the President denounces as lawless, they would never have obtained the informa-tion which the government is now using in order to prosecute ex-Secretary Fall or which justified the President in dismissing Harry Daugherty. Mr. Coolidge demands the placing of restrictions on congressional investigations into the conduct of executive officers which would fatally hamper the investigators in obtaining testimony from unwilling witnesses. If Congress yielded to his demands, the record of administrative officials would thereafter be immune from effective inquiry unless the accused official admitted sufficient evidence of crime to warrant an indictment. ...
>
> The President's final decision to attack the investigators and disparage the practice of investigations is particularly deplorable for one reason. It is always undesirable that select congressional committees should have to

conduct investigations for the purpose of exposing corruption in office rather than for the purpose of seeking methods of improving the administration of the laws. Investigations of this kind would not be necessary, if administrative officials could be trusted to report indications of corruption on the part of their fellow officials; and when they are necessary it is only because a certain number of public employes and private citizens enter into a tacit or overt conspiracy to allow the corruption to continue. When the exposure finally takes place, popular opinion justifiably suspects the existence of such a conspiracy, and its suspicions are bound to increase and to become reckless in so far as any indication exists of a successful plan to suppress the facts and to force the investigators to quit. . . .[8]

Here again there is no evidence of a gift of foresight. That a day might come when investigations by a Senate committee would suggest that a certain number of public employees and private citizens had entered into a different sort of conspiracy; that in such an event constitutional protection of those accused might appear in a different light — none of this seems to have been thought of by the stern and righteous editorialist.

Aesop's frogs disliked their activist stork king, and wished for his inert predecessor. The problem facing a constitutional philosopher in the United States involves a similar alternation in governmental policy. The constitutionalist must construct a theory by which the Supreme Court will keep hands off the other branches of the Federal government, and hands off the States, when aloofness is proper; and yet will act against each at proper times. We must have King Log and King Stork turn and turn about as each role is proper. This is not simple, for opinions on questions of propriety are rarely unanimous.

III

Liberal impatience with the United States Supreme Court in the twenties was mainly based on four of its lines of decision. The most exasperating was the limitation the Court found in the powers of the federal Congress to control the nation's economy. The economic activity of the nation was unitary, making little question of interstate and intrastate. The procession of necessities of life from the sea and the mine and the forest and the farm to the men who used or consumed them went on without much relation to state boundaries. And activity within the borders of one state had

repercussions elsewhere. Extensive factory legislation in New York, in the New England States, and in many others, made it difficult for these to compete in manufacturing with states having a minimum program of social legislation. Yet the Supreme Court, clinging to the undeniable historic fact that the Congress had been entrusted with power to regulate only commerce conducted with foreign nations and among the several states,[9] continued to hold that production of goods was outside the power of the national legislature. Naturally, those who did not wish to be bothered by federal regulation were gratified by this declaration of federal impotence. Conversely, the struggling cause of liberalism called for an expanded power in the national government which could oblige the dissenting minority of states to comply with the ideas of the more progressive majority. But the Supreme Court persisted in the idea that the grant to the Congress of power over commerce between the states meant that power over other commerce was not granted; that the expression of the one excluded the other; and that there must have been some economic activity not entrusted to the national government. Accordingly the Court was vigilant to block evasive devices by the Congress. It found that a congressional attempt to forbid movement of the produce of child labor from state to state was an indirect effort to control conditions of production in factories, and accordingly was beyond the power of the federal Congress.[10] When the Congress then retaliated by imposing a prohibitive tax on the manufacturer who utilized child labor, the Supreme Court saw through this device as well, and struck it down.[11]

A second grievance of liberals arose because, even where legislation was clearly within the scope of the powers delegated to the Congress (when it sought to regulate interstate railroads, or to govern the District of Columbia, for example), Congress was still not free to legislate as it saw fit, for the due-process clause of the Fifth Amendment[12] restricted it. Thus regulation of anti-union discrimination by railroads,[13] and minimum wage legislation in the District of Columbia hospitals[14] were both held invalid, for, said the Court, these statutes deprived those affected of their liberty and property without due process of law.

A third complaint against the Supreme Court was aroused by its restriction of state legislation. Not content with declaring the

Congress to be without power to correct local economic injustice, The Supreme Court had used the clause of the Fourteenth Amendment which forbade any state to " deprive any person of life, liberty, or property, without due process of law," to take from the states, as well, a reasonable power to correct economic and social injustice. The " yellow dog " contract is almost forgotten now; but forty years ago large employers not uncommonly required that a workman, on obtaining employment, agree not to join a labor union during the period of his service. When the Kansas state legislature forbade this restrictive practice, the Supreme Court declared the law invalid under the Fourteenth Amendment.[15] When Kansas attempted to regulate labor matters by establishing a system of compulsory arbitration with a Court of Industrial Relations similar to that in Australia, the Supreme Court likewise declared this to be a deprivation of liberty and property without due process of law.[16] When Tennessee in 1929 attempted to fix the price of gasoline, the Supreme Court held that this move, too, was invalid under the Fourteenth Amendment.[17] New Jersey was forbidden to fix the fees of employment agencies,[18] New York to fix the charges of theater ticket brokers.[19] Oklahoma was forbidden to limit competition in the ice business.[20] The Fourteenth Amendment guaranteed a rather large measure of laissez-faire in the States.

A fourth grievance arose because the Commerce Clause,[21] a grant of power to Congress not expressed as a restriction on the States, was construed by the Supreme Court to prohibit various State commercial regulations, and to limit the power of the States to tax multi-state economic operations. The Court felt the danger that economic parochialism, the creation of what amounted to disguised state protective tariffs, could tend to break up the nation into a multitude of protectionist units, to the danger of the United States as a whole, and it found in the Commerce Clause a not-implausible basis for declaring ineffectual State regulation and State taxation which tended to unreasonable fragmentation of the national economy. But this had the effect of freeing certain large commercial operations from State legislative jurisdiction — at least until the Congress should give its consent to State action, and this consent was not always forthcoming. Here was another occasion for complaints against the federal judiciary.

The opinions of the Supreme Court were not always unanimous, of course. The detachment with which Holmes viewed the human comedy led him to much more tolerance of legislation than the majority of his brethren. The Fourteenth Amendment, he wrote in a famous dissent, was not intended to enact the Social Statics of Mr. Herbert Spencer.[22] Brandeis came to join him; Stone succeeded McKenna in 1925, and the three saw much alike. But throughout the twenties the majority of the Court continued to find much state and some federal legislation unconstitutional, and to draw on its head the criticisms of liberal men.

IV

With the powers of the nation and states thus circumscribed, the United States entered the depression of 1929. There is neither time nor need to repeat here the story of the next three years. The government attempted some remedies. The Reconstruction Finance Corporation and Home Loan Banks foreshadowed great expansion of federal activity to come. The Norris-LaGuardia anti-injunction act of March 23, 1932, expressed a distrust of the decisions of the federal judiciary in labor matters, and tended to promote union activity at a moment when, unhappily, it could accomplish little. The depression deepened. President Hoover's last days in office were passed while banks were closing all over the country.

The Congress was convened in extraordinary session on March 9, 1933, by President Roosevelt. During the next hundred days it passed a remarkable series of recovery statutes, the origins of the New Deal. The judicial history of this legislation and its successor statutes during the next four years has aptly been called by Professor Corwin, " Constitutional Revolution Limited."

The Supreme Court, during this period of revolutionary reversal of attitude, continued without a change of personnel. From March 14, 1932, when Mr. Justice Cardozo took the seat vacated by Justice Holmes, until June 2, 1937, when Mr. Justice Van Devanter retired, the Court consisted of Chief Justice Hughes and Associate Justices Van Devanter, McReynolds, Brandeis, Sutherland, Butler, Stone, Roberts and Cardozo. Overfacile classification of the Justices of the Supreme Court into what are described as liberal and conservative groups is not uniformly

successful. The intellectual predispositions of nine human beings are less easy to sort than a basketful of seashells or a glass case full of stuffed birds; and this insusceptibility to easy classification becomes more apparent as the more complex issues of the forties succeed those of the thirties. In general, however, Chief Justice Hughes and Associate Justices Brandeis, Stone and Cardozo tended to follow the Holmes tradition in recognizing substantially expanded federal powers, and in according to national and state legislatures a considerably greater tolerance under the due-process clauses of the Fifth and Fourteenth Amendments than the majority of the Court had been willing to permit during the decade of the twenties. Justices Van Devanter, McReynolds, Sutherland and Butler by and large continued to take the attitudes which they had taken in the years before the New Deal. Mr. Justice Roberts tended to remain with the conservative group until the spring of 1937.

As constitutional litigation ordinarily takes a rather long time to reach the lower courts, and then to work its way up to the Supreme Court of the United States, a period of two years passed before the first of the New Deal statutes came to grief. Then, however, came a rush of such cases. The years 1935 and 1936 marked the high point of Supreme Court declarations of unconstitutionality of federal measures. On the seventh of January, 1935, the Court in an opinion written by Chief Justice Hughes held unconstitutional two presidential executive orders prohibiting the transportation of petroleum produced in violation of a state law.[23] Six weeks later, passing on the validity of the congressional joint resolution of June 5, 1933, for the payment of United States gold bonds in whatever currency the Congress might make legal tender at the time, the Court held that the bondholder could recover no damages against the United States by reason of devaluation, but only because he could not prove that the paper dollar would buy less than the gold dollar in the United States.[24] The opinion was frank in its statement that the United States had no power to repudiate its promise, and that only the failure of proof of loss prevented the plaintiff from a recovery of damages against the government. On May 6, 1935, the Supreme Court declared unconstitutional the Railroad Retirement Act of 1934.[25] The prevailing opinion of Mr. Justice Roberts found the

statute in conflict with the due process clause of the Fifth Amendment; and also found that it was not actually a regulation of interstate commerce.

However, for the New Deal the blackest day in that spring was May 27, 1935, when the Court handed down three crippling judgments. It held in one case that President Roosevelt had no power to dismiss a federal trade commissioner merely because of policy differences;[26] and in another that a 1934 amendment to the federal Bankruptcy Act impeding the rights of holders of farm mortgages to foreclose was unconstitutional, on the ground that the creditor's interest was thus taken for the benefit of the debtor and that this was a violation of the Fifth Amendment.[27] The hardest blow of the three to the Roosevelt Administration was the decision invalidating the National Industrial Recovery Act.[28] The NRA, administered by that slashing cavalryman, General Hugh Johnson, had formulated codes of fair trade practice, had put the Blue Eagle in shopwindows on every Main Street, had diverted the populace with recovery parades, and had altogether provided a colorful diversion in the long drabness of the depression. When a few elderly justices declared that the whole business had all along been unconstitutional, simply because merchandising was local and not interstate commerce, the letdown was shocking.

On the 31st of May, four days after the decision was handed down, President Roosevelt held a press conference at the White House. Two hundred newspapermen were present, with Mrs. Roosevelt, Senator Robinson, the Democratic leader, and Charles Michaelson, the publicity agent for the Democratic National Committee. The President went over the NIRA opinion in detail. He said that the decision was more important than any other in the lifetime of anyone present. The commerce clause had been written in the Constitution in the horse and buggy days of the eighteenth century when commercial problems were far simpler than those of today and communities were largely self-supporting. Conditions were different now. He said that the issue was whether the country was going to go one way or the other, whether it was going to recognize the right of the federal government to control economic conditions which needed control, or was going to turn back to the state functions of horse and buggy days.[29]

Despite this reverse the President continued the battle for federal power over the national economy. The bituminous coal industry of the country was in a sad state, and federal legislation was pending before the Congress designed to bring some order into soft-coal production, marketing and labor relations. On July 6, 1935, Mr. Roosevelt wrote of this bill to Congressman Hill of the Ways and Means Committee:

> Manifestly, no one is in a position to give assurance that the proposed act will withstand constitutional tests, for the simple fact that you can get not ten but a thousand differing legal opinions on the subject. . . . I hope your committee will not permit doubts as to constitutionality, however reasonable, to block the suggested legislation. . . .[30]

The Congress stilled any qualms it may have had and passed the Act.[31]

The procession of adverse judgments continued. On December 9, 1935, the Court struck down part of the Homeowner's Loan Act of 1933.[32] On January 6, 1936, the Agricultural Adjustment Act of 1933 was declared unconstitutional.[33] In this opinion Mr. Justice Roberts caused some wonderment among constitutional students by writing for the Court:

> When an act of Congress is appropriately challenged in the courts as not conforming to the constitutional mandate, the judicial branch of the Government has only one duty, — to lay the article of the Constitution which is invoked beside the statute which is challenged and to decide whether the latter squares with the former. All the court does, or can do, is to announce its considered judgment upon the question.

No clause of the Constitution forbids in so many words any of the provisions contained in the statute in question. The Court rested its judgment entirely upon the theory that the combination of a tax on processors and benefits to farmers amounted in the aggregate to a control of the amount of crops produced, and that this was beyond the interstate commerce power. It was difficult for critics to understand Mr. Justice Roberts' plan of parallel-column jurisprudence.

Even this was not the last of the sad procession of cases. The Bituminous Coal Conservation Act, passed at the President's urging, was declared invalid under the federal due process and commerce clauses on May 18, 1936.[34] A week later, on May 25, 1936,

a statute designed to make available a modified form of bankruptcy to insolvent municipalities was adjudged unconstitutional on the ground that the United States was unduly interfering with state matters.[35] And on June 1, 1936, although the Court had two years before upheld price fixing in the milk industry in New York,[36] it now apparently turned about and held that a New York statute fixing minimum wages for women was a violation of the Fourteenth Amendment due process clause.[37]

This parade of decisions, made while the depression was still severe, raised criticism of the Supreme Court to a high point. People came to feel that the Court, or at least part of it, was intellectually and emotionally sclerotic. Drew Pearson and Robert S. Allen in 1936 published a book entitled "The Nine Old Men" criticizing the Court and its decisions. The book was neither temperate, nor scholarly, nor just to such a man as Louis D. Brandeis, but it was exciting, and above all had a catchy title. A comment of the authors on the Bituminous Coal decision was:

> Right may have been on the side of the miners, but might was on the side of the operators. Five reactionary justices bent on legislative murder count for more than three liberals, regardless of how righteous their cause and how irrefutable their logic.[38]

Messrs. Pearson and Allen, however, were not as drastic as a Mr. Louis Goldberg and Miss Eleanore Levenson, who in a book called "Lawless Judges" published in 1935 by the Rand School Press in New York, after a critique of the National Industrial Recovery Act case, wrote (pgs. 241, 242):

> ... The recall of judges and judicial decisions must be generally established. People must be aroused to the point of using the recall as one means of protecting themselves against judicial tyranny.
>
> Until the recall of judges and judicial decisions shall become effective, we advocate the impeachment of any judge who deliberately misinterprets a statute or law, and that the process of impeachment be made simpler. ...
>
> We know that it is difficult to strip the ermine from judicial shoulders, but the worshipful attitude of the people towards the courts must be changed through education. ...

The most effective criticism of the Supreme Court was in the election of 1936. President Roosevelt obtained an overwhelming majority. Three months after this popular vindication, he sent to

the Congress a message[39] proposing legislation which, if enacted, would work an immediate change in the complexion of the Supreme Court, and of the lower federal courts. He mentioned the age of federal judges as a disadvantage in their work and pointed to conflicting constitutional decisions in lower federal courts and the delay in getting the cases to the Supreme Court as hampering the federal government. He said:

> Life tenure of judges, assured by the Constitution, was designed to place the courts beyond temptations or influences which might impair their judgments; it was not intended to create a static judiciary. A constant and systematic addition of younger blood will vitalize the courts and better equip them to recognize and apply the essential concepts of justice in the light of the needs of the facts of an everchanging world.

With the message the President sent a draft of a bill[40] providing that when any federal judge on life tenure reached the age of seventy, had been in office ten years, and within six months had neither resigned nor retired, the President should appoint an additional judge to the same court. Various limiting clauses were written in the draft, including a provision that the members of the Supreme Court should not exceed fifteen.

This proposal produced a surprisingly hostile reaction, both in and out of Congress. Opponents of the measure called it the " court-packing bill," borrowing a term from the trial bar where " jury-packing " is the intentional introduction of prejudiced jurors into the jury-box. Despite its opinions in constitutional cases, the Supreme Court turned out to be widely revered, and the " packing " proposal appeared to debase it. The American Bar Association Journal for 1937 is largely taken up with articles discussing the plan, and, while some authorities supported it (Professor Thurman Arnold of Yale Law School, to mention only one prominent example), the preponderance of opinion was hostile. The President's proposal was never adopted by the Congress.

Much has been written on the effect of the court-packing plan on the justices. The full story of their mental operations can never be known. However, on the 29th of March, 1937, less than two months after the President's message, the Court passed on a minimum wage law of the State of Washington, substantially like the New York statute which had been invalidated under the Fourteenth Amendment due process clause the preceding year.[41]

Mr. Justice Roberts, who had voted against the New York statute ten months before, now joined what has been described as the liberal side of the Court, leaving the four conservative justices — Sutherland, Van Devanter, McReynolds, and Butler — in a dissenting minority. This change of opinion upheld the Washington law, and was prophetic of a greater change. A few days later on April 12, 1937, the National Labor Relations Act was upheld as applied to a steel mill.[42] The division of the justices was the same; Justice Roberts voted to uphold the law. It was apparent that the conception of the federal power under the commerce clause had been drastically revised, and was to be applied thereafter to much that had for many years been considered purely local. And due process no longer was inconsistent with legislative innovation in the economic field. The constitutional revolution had occurred.

V

The year 1936 divides the period since the decision of the Child Labor Case[43] into two roughly equal parts. The Supreme Court, since that significant middle date, has represented in the eyes of American liberal opinion something entirely different from the Supreme Court of the preceding eighteen years. All the justices of the 1932-1936 Court have left the bench. Of the four doctrines of the twenties which gave pain to political and academic critics, three have ceased from troubling. The commerce power is no longer inadequate for any purpose desired by the most enthusiastic Hamiltonian Democrat; the Court has found the Congress acting within its rights in authorizing the Secretary of Agriculture to fix the amount of wheat a farmer can grow for consumption on his own farm;[44] and has upheld a federal statute under which there were fixed the wages of elevator operators in an office building. Interstate commerce is now vertical as well as horizontal. So inclusive have the powers of the National Labor Relations Board proved to be, that that body has been obliged to adopt a set of rules limiting its own undertakings lest its calendars be littered with a mass of business concerning unionization of corner drugstores and the like.[45] The Supreme Court has held no congressional statute invalid as outside the commerce power since 1936.

Nor, with one possible exception, has the due-process clause of

the Fifth Amendment restricted any federal economic legislation since 1936. On December 8, 1952, the Court held fatally vague a section of an act of Congress making it a crime for a factory owner to refuse an official of the Food, Drug and Cosmetic Administration to enter his premises. And the ground of this decision is significant. Justice Douglas in his opinion compares the section under attack to the laws of Caligula, — of whom it is written —

> . . . inasmuch as many offenses were committed through ignorance of the letter of the law, he at last, on the urgent demand of the people, had the law posted up, but in a very narrow place and in excessively small letters, to prevent the making of a copy.[46]

Here it is not easy to say whether the Court is protecting " property rights or human rights." Here, perhaps, the troublesome problem of today's Court is exemplified. It is not easy here to choose the liberal side of the case.

Since the new dispensation of 1937, the Court has declared only two other acts of Congress unconstitutional.[47] One was a statute forbidding payment of salary to Robert Morss Lovett and two other federal employees, who had been the subject of a subversion investigation by a congressional committee. In 1946, in an opinion by Mr. Justice Black, this was held unconstitutional as a bill of attainder forbidden by Article I, Section 9, Clause 3. The other federal statute, upset in 1943, attempted to make it a crime for a man with a criminal record to receive firearms which had been shipped in interstate commerce, and made possession of the arms presumptive evidence of interstate origin. In 1943 the Court, in an opinion by Justice Roberts, held this presumption so violent as to exceed the bounds of due process of law.

During the last sixteen years the Court has been much more severe on State legislation than on acts of the Congress, but it has declared invalid very little economic regulation. As was the case before 1937, the constitutional clause it relies on is that part of the Fourteenth Amendment which forbids any State to deprive any person of life, liberty, or property without due process of law. It has forbidden Illinois to permit religious instruction of children in school buildings during school hours when attendance is compulsory.[48] It has forbidden New York to penalize the publication of accounts of " bloodshed, lust, or crime " on the ground that the

statute is too vague to enforce,[49] and has forbidden the same State to shut down the exhibition of a motion picture film as "sacrilegious."[50] It has struck down restrictions imposed by States on the rights of the Negro to vote,[51] to attend a university[52] and a professional school.[53]   It has forbidden a state to convict a defendant of serious crime without the advantage of advice of counsel,[54] to extract from a defendant testimony against his will,[55] and to try a defendant in secret.[56] It has forbidden Illinois to penalize a speech solely because it "stirs the public to anger, invites dispute, brings about a condition of unrest, or creates a disturbance."[57] It has forbidden New York to require a license as a condition of preaching in the streets.[58] On November 24, 1952, it forbade New York to enforce a statute purporting to turn over the temporalities of one warring branch of a Russian church to another branch on the ground that it interfered with the free exercise of religion.[59] These cases, one notes, tend to the protection less of "property rights" (if one can make this distinction) than of "human rights."

The "human rights" here protected against the States — freedom of expression, freedom of religion, freedom from unreasonable rigor in criminal justice — are all included in the phrase "life, liberty, or property," and the State is restricted from depriving any person of these rights without "due process of law." A difficulty with this language is its vagueness. It says nothing about Russian churches, street preachers, or magazines full of bloodshed, lust or crime. The Justices have to find these things buried in "life, liberty, or property," and they have to discover that whatever the State did took away these rights without "due process." Some of the Justices are troubled because these same words were used under the *ancien régime* to prevent Tennessee from fixing the price of gasoline, and to invalidate other like State measures.

This ambiguous language of the Fourteenth Amendment has aroused one of the notable differences of theory in the present Court. Mr. Justice Black remembers the bad old days, and sees them threatening still. He wishes for definition in the language of the due-process clause, so that it will, by the same words, protect human freedom but not protect economic selfishness. In 1947 the Court had before it the case of a man named Admiral Dewey Adamson,[60] who was charged with murder in California. In that

State, under certain circumstances, a judge and prosecutor may comment on the failure of a defendant to take the witness stand and explain the evidence against him; and Adamson was convicted after such proceedings. The Court upheld this statute; but in a dissent, Mr. Justice Black protested against what he described as:

a constitutional theory . . . that this Court is endowed by the Constitution with boundless power under "natural law" periodically to expand and contract constitutional standards, to conform to the Court's conception of what at a particular time constitutes "civilized decency" and "fundamental liberty and justice."

He went on to say that the Fourteenth Amendment had been intended to effect a sort of shorthand incorporation by reference of all the prohibitions against federal action contained in the Bill of Rights — no more and no less — including freedom from the sort of compulsory self-incrimination California used on Adamson. In this way, he thought, all the freedoms of the first eight amendments would be available against the States as well as the federal government, and yet the danger of economic vetoes would be eliminated.

There are two difficulties with this theory of Mr. Justice Black. One is that the historical evidence of the intention he ascribes to those who adopted the Fourteenth Amendment is not entirely clear. The other objection is that the language of much of the Bill of Rights is ill adapted to restrictions on the States; and as the Fifth Amendment itself contains a clause prohibiting deprivations of "life, liberty, or property without due process of law," Mr. Justice Black in getting rid of one ambiguity would incorporate by reference its identical twin.

On the other hand, the alternative is not simple.

On January 2, 1952, Mr. Justice Frankfurter expressed himself forcibly about the Black theory of incorporation by reference.[61] He said, among other things:

In dealing not with the machinery of government but with human rights, the absence of formal exactitude, or want of fixity of meaning, is not an unusual or even regrettable attribute of constitutional provisions. Words being symbols do not speak without a gloss. On the one hand the gloss may be the deposit of history, whereby a term gains technical content. . . . On the other hand, the gloss of some of the verbal symbols of the Constitution does not give them a fixed technical content, it exacts a continuing process of application.

When the gloss has thus not been fixed but is a function of the process of judgment, the judgment is bound to fall differently at different times and differently at the same time through different judges. . . . The vague contours of the Due Process Clause do not leave judges at large. We may not draw on our merely personal and private notions and disregard the limits that bind judges in their judicial function. Even though the concept of due process of law is not final and fixed, these limits are derived from considerations that are fused in the whole nature of our judicial process. . . . Due process of law thus conceived is not to be derided as a resort to a revival of "natural law." To believe that this judicial exercise of judgment could be avoided by freezing "due process of law" at some fixed stage of time or thought is to suggest that the most important aspect of constitutional adjudication is a function for inanimate machines and not for judges, . . .

Mr. Justice Black views this looseness with alarm. He answers Mr. Justice Frankfurter in a special concurring opinion:

There is, however, no express constitutional language granting judicial power to invalidate *every* state law of *every* kind deemed "unreasonable" or contrary to the Court's notion of civilized decencies; yet the constitutional philosophy used by the majority has, in the past, been used to deny a state the right to fix the price of gasoline, Williams v. Standard Oil Co., 278 U. S. 235; and even the right to prevent bakers from palming off smaller for larger loaves of bread, Jay Burns Baking Co. v. Bryan, 264 U. S. 504. These cases, and others, show the extent to which the evanescent standards of the majority's philosophy have been used to nullify state legislative programs passed to suppress evil economic practices. What paralyzing role this same philosophy will play in the future of economic affairs of this country is impossible to predict.

The Black-Frankfurter difference of theory goes to the heart of the problem facing one who contemplates constitutional government in the United States today. The majority is entitled to have its way. But what if the majority is wrong? By what criterion shall right be determined if not by fifty-one percent of the votes? By the Constitution? What words in it? The Fourteenth Amendment says nothing about Russian cathedrals or magazines of bloodshed, lust or crime. In what narrow place is this Constitution posted up? In what small letters is it written?

VI

The possibility of majority error poses a problem not limited to our own time nor to our own country, nor to constitutional law. For centuries men have tried to devise a form of words which will

so describe error as to make it recognizable. Defined in religious terms, as a violation of the law of nature for which another name is the will of God, it becomes philosophically neat. Says John Locke:

> Thus the law of nature stands as an eternal rule to all men, legislators as well as others. The rules that they make for other men's actions must, as well as their own, and other men's actions be conformable to the law of nature, i.e. to the will of God, of which that is a declaration, and the fundamental law of nature being the preservation of mankind, no human sanction can be good or valid against it.[62]

This satisfies the need of a pamphleteer who seeks to justify a glorious, and of course successful, revolution better than it does the need of a judge who is asked by a litigant to disregard a statute. Despite the respect which a beneficiary of the Declaration of Independence owes to the Laws of Nature and of Nature's God, he must concede that these have never been codified sufficiently to serve as practical criteria of constitutionality. Can France of the enlightenment offer any more definite guide?

Rousseau, nearly two centuries ago, saw men joining in a social compact to form a nation governed by what he calls the " general will," which by his definition is always right and tends always to the public benefit. Granted only a well-informed citizenry and no communication among citizens (says the displaced Genevese), what small errors might arise in this or that man would cancel themselves out; the result of popular deliberation would then always be good.[63] But the misguided people will persist in talking to one another; and they are incurable joiners. Parties, associations grow up; by and by one group grows so powerful that it carries the day over all the others. The result, unhappily, is no longer the general will.[64] Dwellers in big cities are peculiarly subject to being thus led astray. An adroit crook, a slick talker, can get his way with the people of Paris or London when the Bernese or Genevese, more rustic and therefore less gullible, would run him out of town.[65]

Majorities, that is to say, can err like monarchs. Fifty-one percent of the populace expressing their will through fifty-one percent of the legislators still may not achieve Rousseau's ideal volonté générale — this built-in correctness of mankind-if-it-doesn't-make-mistakes. Such an enviable posture of affairs is, one

gathers, achieved only by a citizenry which, having first renounced life in great cities, then deliberates on public affairs with adequate information but without communications between citizens. Lacking these essential and somewhat infrequent conditions for automatic freedom from majoritarian error, the United States has turned to the Supreme Court and Due Process of Law.

Holmes in 1905[66] tried his hand (and a skilled one it was) at defining the sort of majority mistakes which are so bad as to make a state statute invalid. " I think," he wrote, " that the word liberty in the Fourteenth Amendment is perverted when it is held to prevent the natural outcome of a dominant opinion, unless it can be said that a rational and fair man necessarily would admit that the statute proposed would infringe fundamental principles as they have been understood by the traditions of our people and our law." Habit, then, has something to do with rightness. The " traditions of our people " must not too intemperately be offended by a transient majority. Rightness has some connotations of national history.

Justice Frankfurter follows his great precursor in stating criteria of that which is rightful. It can be tested, he has said, by " that feeling of just treatment which has been evolved through centuries of Anglo-American constitutional history and civilization,"[67] and " the notions of justice of English-speaking peoples "[68] (though one justice recently expressed " doubt as to why we should consider only the notions of English-speaking peoples to determine what are immutable and fundamental principles of justice ").[69] Traditions cannot of course become so fixed as to prevent change. Habit cannot deprive the States of opportunity for reforms in legal process. "Law must be stable and yet it cannot stand still."[70] Whoever has sympathetically followed the struggles of men to describe rightness in a phrase must have become convinced of the impossibility of the task. The jesting Pilate was wise not to stay for an answer. Before he found out what truth was, he would have had a long wait.

Attempts to specify rightness in terms so precise as to decide specific debatable cases are doomed in advance. The effort indicates a misconception of the judge's task, to which habitual forms of speech give currency. We like to speak of ourselves as governed " by laws and not by men," as though somehow the detailed rela-

tions of individual man to collective mankind had been catalogued in print; as though what is required of the good judge is proficiency in the use of an index. This illusion vanishes as soon as one makes an attempt to find the applicable rules of conduct in any considerable number of instances. Most rules, to be sure, as applied to most situations are clear, and are accepted as commonplace. In general, laws against homicide or the intentional destruction of other men's property cause little constitutional debate.[71] But the application of great principles like the desirability of free expression, the accessibility of political change, the opportunity of a man to make protest before the force of government is turned against him, immediately raise questions for which no book can provide an answer. A young man is making a somewhat denunciatory political speech on a city sidewalk; a crowd collects; some threats of violent interference are made among the spectators; the passage of traffic may be interrupted. Can the city police, consistently with the urge for open talk latent in the due process clause of the Fourteenth Amendment, arrest the young man?[72] The concept that one who violates a law forbidding revolutionary exhortation must still go unpunished unless the gravity of the evil discounted by its improbability justifies such invasion of free speech[73] requires for its application a discriminating statesmanship; the answer cannot be looked up in a treatise. When a group of Negroes picketed a store to induce employment of more Negroes by the management, the propriety of a State injunction against the picketers could not be determined by any pat formula expressing predominance of human rights over property rights.[74]

No one really wants an inflexible set of rules to live by. When a demand for certainty is heard, it is apt to be, at base, a demand for some different result in a specific case. And when a rule is so definitely expressed in the laws that its application is unavoidable, one is apt to hear complaints that in some specific instance the rigidity has produced an " unjust " result — a comment which postulates a rule of justice transcending particular legislative formulation.

What really happens is that legislation (in a constitution or in some lesser law) often expresses a rather broad policy, and we expect the judge to carry out that policy in such a way as to achieve what is right in each case. Admittedly this feeling of

"rightness" will derive from the judge's preconceptions, habits, previous experience. The same sort of influences govern the policy decisions of the legislator and the administrator. But we hope that our judges can have a wider mass of governing relevancies than the legislator and the administrator. One of the wisest judges of our time has said that a judge who would pass on constitutional questions should be on bowing terms with philosophers and historians and poets, for supple institutions are not shaped by judges whose outlook is limited by parish or class.

> They must be aware that there are before them more than verbal problems; more than final solutions cast in generalizations of universal applicability. They must be aware of the changing social tensions in every society which make it an organism; which demand new schemata of adaptation; which will disrupt it, if rigidly confined.[75]

And why should not Presidents and Congressmen be required, likewise, to be nicely poised philosophers? One doubts the merit of the prescription. The capacity of the legislator to reflect impatient and perhaps transient popular emotion; the willingness of the executive to damn the torpedoes and go ahead — these are as necessary to make up the compound of government as the thoughtful search for justice.

Is our highest law amorphous, without structure, unascertainable? Must counsel for business corporations, to learn to advise their clients, read historians and philosophers and poets? By and large the answer is that if the question at issue is important, catholicity of learning is apt to be useful. In the most significant constitutional controversy in which great corporations have been involved since 1937 — the Steel Seizure Case of June, 1952[76] — the opinions of the justices cited the Federalist Papers, the autobiography of Theodore Roosevelt, a biography of President Taft, the Works of Alexander Hamilton, correspondence of Abraham Lincoln, Woodrow Wilson's "Constitutional Government," Churchill's "The Unrelenting Struggle," Rossiter's "Constitutional Dictatorship," Holdworthy's "History of English Law," correspondence and a Message to Congress of President Jefferson, correspondence of President Madison, correspondence of John Jay, and the *Economist* of May 10, 1952, which observed:

> At the first sound of a new argument over the United States Constitution and its interpretation the hearts of Americans leap with a fearful joy. The

blood stirs powerfully in their veins and a new lustre brightens their eyes. Like King Harry's men before Harfleur, they stand like greyhounds in the slips, straining upon the start.

Despite these literary and historical riches, constitutional law is not without structure. While undeviating adherence to precedent is not observed and probably is undesirable in view of changes of circumstances with the passage of time, still within the wide boundaries of such general policies of governmental fairness as the Due Process Clauses, patterns of decision form and become reasonably stable so that some measure of predictability is achieved, except in those marginal questions which will always be present when any division line is drawn. Mr. Justice Stone set one such pattern in 1938 when he suggested that "legislation which restricts those political processes which can ordinarily be expected to bring about repeal of undesirable legislation, is to be subjected to more exacting judicial scrutiny under the general prohibitions of the Fourteenth Amendment than are most other types of legislation."[77] State statutes infringing freedom of expression, then, are declared unconstitutional more readily than state economic laws; the history of the Court's decisions since 1937 supports this grouping. And if within Stone's class individual cases are sometimes decided for one side and sometimes for the other, this indicates no judicial challenge to the principle of free expression; it only shows that in the infinite mutability of human affairs, judicial decisions on what are actually complicated questions of fact[78] will sometimes bring a case within the principle and sometimes not. Outcry at a given decision on constitutionality may be in words directed against an asserted abandonment of principle, or against the power itself of courts to declare statutes unconstitutional. Actually the outcry is apt to represent only a feeling that in this instance the Court had mistakenly decided a complex matter of fact.

Despite the indefiniteness of the criteria of unconstitutionality, judicial inaction is not today a slogan for liberals. In these times it sometimes seems less certain than it was before 1937 that mankind, left to itself, will achieve desirable laws. We are told that adroit rascals, insinuating haranguers, such as Rousseau saw swaying the people of London or Paris, are abroad in the land cozening the majority into the legislation of evil. Self-restraint by the

Court has ceased to be an unqualified virtue. It is desirable only when the laws are good. When the Legislature is wrong, judicial inaction is deplorable. King Log is out of favor on the campus.

In the magazine *Look* for July 31, 1951, Professor Rodell of the Yale Law School, writing under the title of " Our Not So Supreme Court," complains of the refusal of that body to take jurisdiction of more cases. He even finds kind words for the pre-1937 Court.

> The Court of Nine Old Men, for all its conservatism, [he writes] had the courage and integrity to take on the tough cases and meet squarely the important issues that elicited Holmes' and Brandeis' famous dissenting opinions.

Professor Eugene Rostow of Yale, in a scholarly paper in the December, 1952, *Harvard Law Review*, strikes a note that would have sounded discordant to the editorialists of the *New Republic* of 1924. He says:

> The idea that judicial review is undemocratic is not an academic issue of political philosophy. Like most abstractions, it has far-reaching practical consequences. I suspect that for some judges it is the mainspring of decision, inducing them in many cases to uphold legislative and executive action which would otherwise have been condemned. Particularly in the multiple opinions of recent years, the Supreme Court's self-searching often boils down to a debate within the bosoms of the Justices over the appropriateness of judicial review itself.
>
> The attack on judicial review as undemocratic rests on the premise that the Constitution should be allowed to grow without a judicial check. The proponents of this view would have the Constitution mean what the President, the Congress, and the state legislatures say it means. In this way, they contend, the electoral process would determine the course of the constitutional development, as it does in countries with plenipotentiary parliaments.
>
> But the Constitution of the United States does not establish a parliamentary government, and attempts to interpret American government in a parliamentary perspective break down in confusion or absurdity. . . .

Two great types of questions are continually coming before the Court — entertained there, so far as can readily be seen, by the will of the nation. One of these involves the decision whether a given activity transcends the allotted functions of the States in a federal structure — whether, for example, the State is unduly taxing national commerce. Another type concerns controversies over the adjustment of relations between the individual and his State,

under guarantees by the United States that no State shall treat its people too badly; and controversies between the individual and that great, remote, and somewhat slow-moving entity, the United States itself. In December, 1952, there was left to the Court to decide as important a question of adjustment of power between nation and State, and of the fundamental decency of the State toward a large body of its citizens, as has arisen in living memory — the question whether it is consistent with the right of everyone to liberty and equal treatment from the State that in public schools some children be secluded from others because of the color of their skins. There is nothing in the words of the Fourteenth Amendment to tell the Supreme Court how to decide this question. Only ideas of fairness will guide it in the last analysis. And if, to some of us, the answer to this question on grounds of fairness seems reasonably clear, the clarity disappears in other issues which every year are submitted to the federal judiciary.

As this paper is written, there are pending before the Supreme Court for argument or decision cases concerning the standing of aliens who claim a right to stay in the United States; cases involving the rights of the accused in criminal trials in state courts; cases involving rights of employees under the National Labor Relations Act; a case involving primary voting by Negroes in Florida. There is a case in which the Court must decide complex questions as to which of the several States is the proper one to decide matters of family law such as divorce or custody of children when the family is scattered and moving about. There is a dispute over the control of electric power generated in one State but utilized in several. There is a dispute over the propriety of railroad freight rates fixed by the Interstate Commerce Commission. Before the Court, week by week, comes an endless variety of controversies arising from our overwhelming growth of population and our complicated national structure. It is conceivable that we might have a nation without the federal judiciary. But we should have to have the same functions performed by another organ, and probably it would resemble what we now have. If the Court did not exist, we should have to create one. Some sort of Supreme Court is going to be continued as long as the people of the nation have creeping doubts of the proposition that the majority view is right all of the time. Our Court is the symbol of our people's self-restraint.

NOTES

[1] " Du Contrat Social," Book II, Chapter 3.

[2] Holmes, " Natural Law," Collected Legal Papers, 310.

[3] Cf Guido de Ruggiero, " Liberalism," V Encyclopedia of Social Sciences, 435, 440.

[4] Speech before the American Federation of Labor at Cincinnati, June 21, 1922, reprinted in Congressional Record Vol. 62, p. 9077.

[5] Colegrove v. Green, 328 U. S. 549 (1946).

[6] South v. Peters, 339 U. S. 276 (1950).

[7] *New York Times*, April 12, 1924, page 1.

[8] Mr. Albert Mavrinac, now of the Department of Government at Wellesley, is the author of a brilliant study, soon to be published, of the shift of editorial policy in the United States during the last generation. I am indebted to him for the references to the President's message and the *New Republic* editorial.

[9] U. S. Constitution, Art. I, Sec. 8, Cl. 3, " The Congress shall have Power . . . To regulate Commerce with foreign Nations, and among the several States. . . ."

[10] Hammer v. Dagenhart, 247 U. S. 251 (1918).

[11] Bailey v. Drexel Furniture Co., 259 U. S. 20 (1922).

[12] " No person shall . . . be deprived of life, liberty, or property, without due process of law. . . ."

[13] Adair v. U. S., 208 U. S. 161 (1908).

[14] Adkins v. Children's Hospital, 261 U. S. 525 (1923).

[15] Coppage v. Kansas, 236 U. S. 1 (1915).

[16] Wolff Pkg. Co. v. Court of Industrial Relations, 262 U. S. 522 (1923).

[17] Williams v. Standard Oil, 278 U. S. 235 (1929).

[18] Ribnik v. McBride, 277 U. S. 350 (1928).

[19] Tyson v. Banton, 273 U. S. 418 (1927).

[20] New State Ice Co. v. Liebmann, 285 U. S. 262 (1932).

[21] See footnote 9 above.

[22] Lochner v. New York, 198 U. S. 45, 75 (1905).

[23] Panama Refining Co. v. Ryan, 293 U. S. 388 (1935).

[24] Perry v. U. S., 294 U. S. 330 (1935).

[25] Railroad Retirement Board v. Alton Railroad Co., 295 U. S. 330 (1935).

[26] Humphrey's Executor v. U. S., 295 U. S. 602 (1935).

[27] Louisville Bank v. Radford, 295 U. S. 555 (1935).

[28] Schechter Corp. v. U. S., 295 U. S. 495 (1935).

[29] *New York Times*, June 1, 1935, p. 1, col. 8.

[30] Published Papers of F. D. R., Vol. IV, pp. 297, 298.

[31] Bituminous Coal Conservation Act of 1935, August 30, 1935, 49 Statutes at Large 991.

[32] Hopkins Fed. Ass'n v. Cleary, 296 U. S. 315 (1935).

[33] U. S. v. Butler, 297 U. S. 1, 62 (1936).

[34] Carter v. Carter Coal Co., 298 U. S. 238 (1936).

[35] Ashton v. Cameron County Water Imp. Dist., 298 U. S. 513 (1936).

[36] Nebbia v. New York, 291 U. S. 502 (1934).

[37] Morehead v. Tipaldo, 298 U. S. 587 (1936).

[38] "Nine Old Men," Ch. XVI, p. 313.

[39] Cong. Record, Feb. 5, 1937, Vol. 81, p. 877.

[40] Ibid., p. 880; U. S. Law Week Supp. 9 Feb. 1937.

[41] West Coast Hotel Co. v. Parrish, 300 U. S. 379 (1937).

[42] NLRB v. Jones & Laughlin Steel Corp., 301 U. S. 1 (1937).

[43] Hammer v. Dagenhart, 247 U. S. 251 (1918).

[44] Wickard v. Filburn, 317 U. S. 111 (1942).

[45] 16 Annual Report of NLRB for the fiscal year ending June, 1951, pp. 15 & 16.

[46] Suetonius, "Lives of the Caesars," Book IV, Chap. 41, p. 469 (Loeb edition).

[47] There have been several additional cases in which federal statutes were narrowly construed to avoid unconstitutionality. Such construction cases approach constitutional rulings.

[48] McCullom v. Board of Education, 333 U. S. 203 (1948).

[49] Winters v. New York, 333 U. S. 507 (1948).

[50] Burstyn v. Wilson, 343 U. S. 495 (1952).

[51] Smith v. Allwright, 321 U. S. 649 (1944).

[52] McLaurin v. Oklahoma State Regents, 339 U. S. 637 (1950).

[53] Sweatt v. Painter, 339 U. S. 629 (1950).

[54] Gibbs v. Burke, 337 U. S. 773 (1949).

[55] Rochin v. California, 342 U. S. 165 (1952).

[56] Re Oliver, 333 U. S. 257 (1948).

[57] Terminiello v. Chicago, 337 U. S. 1 (1949).

[58] Kunz v. New York, 340 U. S. 290 (1951).

[59] Kedroff v. St. Nicholas Cathedral, 344 U. S. 94 (1952).

[60] Adamson v. California, 332 U. S. 46 (1947).

[61] Rochin v. People of California, 342 U. S. 165, 169 (1952).

[62] Second Treatise of Civil Government, Ch. XI, 135 (1689).

[63] Contrat Social, Book 2, Ch. 3, " Si, quand le peuple suffisamment informé délibère, les Citoyens n'avoient aucune communication entre eux, du grand nombre de petites différences résulteroit toujours la volonté générale, et la délibération seroit toujours bonne."

[64] Book 2, Ch. 3, " Si la Volonté Générale Peut Errer." Madison in the Federalist No. 10 repeats this idea in terms suggestive of Rousseau's.

[65] Book IV, Ch. 1.

[66] Dissenting in Lochner v. New York, 198 U. S. 45, 76 (1905).

[67] Frankfurter, J., in Joint Anti-Fascist Refugee Committee v. McGrath, 341 U. S. 123, 162 (1951).

[68] Frankfurter, J., in Malinski v. New York, 324 U. S. 401, 417 (1945).

[69] Black, J., concurring in Rochin v. California, 342 U. S. 165, 176 (1952).

[70] Pound, " Interpretations of Legal History," page 1.

[71] The merits of a claim of governmental responsibility under the Fifth Amendment for the army's destruction of property in the Philippines to keep it from enemy hands caused the Supreme Court worry in December, 1952. U. S. v. Caltex, 344 U. S. 149 (1952).

[72] In Feiner v. N. Y., 340 U. S. 315 (1951), the Supreme Court decided against such a speaker.

[73] Learned Hand, J., in Dennis v. U. S., 183 F 2d 201, at p. 212, adopted in the opinion of Vinson, C. J., 341 U. S. 494, 510 (1951).

[74] Hughes v. Superior Court, 339 U. S. 460 (1950).

[75] Learned Hand, " Sources of Tolerance," 79 U. of Pa. L. Rev. 1, 12 (1930).

[76] Youngstown Sheet & Tube Co. v. Sawyer, 343 U. S. 579, 594 (1952).

[77] See his celebrated Footnote 4, in U. S. v. Carolene Products, 304 U. S. 144, 152 (1938).

[78] See the expression of this idea in Brandeis' dissent in Burnet v. Coronado Oil and Gas Company, 285 U. S. 393, 410 (1932).

VOLUME 80

Contributions to the Analysis and Synthesis of Knowledge

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A consideration of the neurophysiological mechanisms of speech and some educational consequences

WILDER PENFIELD

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WILDER PENFIELD

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The great difference between man and the other mammals is that man can learn to speak and to understand the meaning of speech. It is by means of the spoken and the written word that the ignorance of childhood is changed into the understanding thought of educated men and women.

Bees have a language, as Karl von Frisch has described so entertainingly. Dogs, monkeys, birds communicate with their own kind. But they use a language which is largely inherited, inborn.

Man has a spoken language which must be taught to each growing child. His ability to learn to speak depends upon the postnatal organization of speech mechanisms within his brain. When a baby comes into the world the speech areas of the cerebral cortex are like a clean slate, ready to be written upon. Other forms of movement and sensation, on the other hand, depend on inborn mechanisms which resemble those of other mammals.

When the gross structure of the human brain is compared with that of animals, no new speech organ is evident. The brain of the dog resembles ours in general organization, and that of the chimpanzee still more so. All the lobes which form its major subdivisions are present. The pathways of muscular control are like ours. They have large areas of cerebral cortex devoted to seeing and hearing and feeling and even larger areas for smelling.

Animals learn from experience, as we do, and they remember what they have learned. More than that, they seem to have unique

racial memories which guide them in their behaviour, especially at crucial times in their lives. This memory, which we call instinctive, is strangely lacking in us.

If we did not teach, and if we could not learn, we would be more helpless than all other mammals. In the observations which follow it is my purpose to question the accepted methods of teaching foreign language. I venture to do this, not because I claim any specialized knowledge of pedagogy, but from the point of view of familiarity with the cerebral speech mechanisms and their derangements.

Speaking, and the understanding of speech, also reading and writing depend upon the employment of certain specialized areas of the cerebrum. There is an optimum age when these special areas are plastic and receptive, and my purpose in this rambling, pseudo-scientific discussion is merely to remind educators of this fact.

In the history of our race, we know little of the beginnings of speech. It has been said that it was when man learned to cultivate grain in the valleys of the Nile and the Euphrates that the birth of civilization was made possible. But there was a more important prerequisite to that civilization — the learning of a language. The writing of that language came later, and its evolution can be traced in the tablets of Mesopotamia.

I remember a day when I stepped out of the blazing sun of a street in Baghdad into the Iraq Museum. There, in the museum's half light, I saw rows of clay tablets, each of them shaped like a piece of soap. On them could be seen the stages in the story of writing from picture-drawing and counting to alphabet.

Seton Lloyd, then Curator of the Museum, picked up a tablet and held it in the palm of his hand just as some Sumerian scribe must have held it when it was soft clay more than 5000 years ago. He pointed out the pictograph of a bag of barley and the single mark of the stylus which stood for the number ten (the number of a man's fingers) and the different marks which were used to represent ones. Here was a record of sale at a time when men had learned to cultivate grain in that fertile plain, a time when speech was probably quite well developed and when man was about to turn from pictograph to writing, an evolution which seems to have been relatively rapid.

Today we have learned to understand something of the cerebral mechanisms which enabled man to begin to speak, and later to write and read. I shall describe these in simple outline.

The two cerebral hemispheres of man are homologous. They are mirror images of each other. The cerebral cortex, which covers each hemisphere, is identical in cellular structure when the two sides are compared.

Animals employ each hemisphere for movement and sensation on the opposite side of the body, as we do. Nerve impulses which utilize these twin hemispheres come from central or centrencephalic gray matter in the brain stem that joins the two. But the function of each is like that of the other.

When a child begins to speak, there develops a functional specialization in one cerebral hemisphere, normally the left hemisphere where the right hand is also represented. There are separate areas of the cortex on this, the dominant, side which come to be devoted to the formulation and the understanding of speech. Meanwhile the slate continues to be blank on the right side. Curiously enough, the areas of the right hemisphere, which are homologous to those used for speech on the left, seem to serve no discoverable purpose, while those areas remain intact on the left side.

The cortical representations of speech are indicated by stippled areas in Figure 1. We have been able gradually to work out their more exact position during a succession of operations under local anaesthesia when cortical excision was being considered in the treatment of focal epileptic attacks. We have learned that, if these areas are avoided, removal of seizure-producing cortex will not result in speech paralysis or aphasia.

The areas have been identified by using a gentle electrical current to produce local interference in the cortex while the patient is talking and naming objects shown to him by one of my associates.*

By the electrical interference method in conscious patients, it

*At the present time, Dr. Lamar Roberts and the writer are analyzing about 575 such cases in a study to appear shortly. We have had the help of Mr. R. W. Sparks, speech therapist. An earlier study of such cases was made by Dr. Preston Robb, and those in which the frontal lobe was involved were published (1948). Penfield and Rasmussen have recorded evidence regarding vocalization and arrest of speech (1949; also 1950).

is possible to recognize in the dominant hemisphere three essential areas of cortex devoted to what might be called the ideational aspects of speech, and one such supplementary area (Fig. 1). The frontal speech area is quite separate from the parietal and temporal speech areas. It is possible that the latter two may be continuous. It has not been feasible to explore the depths of the intervening fissures to decide this point. The posterior limits of these two areas also cannot be sharply drawn, but the anterior frontiers can.

Injury to any one of these three areas produces a defect in the ideational mechanisms of speech, or aphasia. Removal of the cortex between the frontal and parietal speech areas does not produce aphasia, although it does produce awkwardness of speaking for reasons that will be apparent presently.

Curiously enough, as Dr. Roberts has pointed out, interference with one cortical speech area produces aphasia which differs little in pattern from that produced by injury of any of the others. Even the supplementary speech area, which may be removed with no more than transient aphasia, produces, when an electric current is applied to it, a disturbance of the ideational mechanisms of speech which resembles that produced by electrical interference with the others.

One must conclude, then, that the organization and coordination of the speech mechanism are carried out by neurone connections that are subcortical but within the same hemisphere.

The ideational speech mechanism is within the dominant hemisphere. It is somewhat like the movement of a watch in the following respect. Interference with one of its cortical parts causes it to stop or to slow down, although each cortical part may well play a very different role in the perfect functioning of this mechanism.

But there is more to speaking than the ideational process involved. There is articulation produced by control of mouth, lips and tongue, and there is vocalization produced by control of the larynx and the muscles of respiration, including the diaphragm. There are four areas of the cortex from which vocalization can be produced by stimulation of the human cortex, the lower precentral, or pre-Rolandic, gyrus of each hemisphere and the supplementary motor area on each side. This is shown in Figure 1 for the left hemisphere, but the right would be the same in this regard.

It seems likely that each of these cortical areas is able to activate by its connections a common mechanism in the brain-stem, for, from each area, the patient is caused to make a similar involuntary cry, a long-drawn-out vowel sound.

Stimulation of the cortex of the chimpanzee, the dog, or the cat does not produce vocalization, although these animals are able to vocalize each in its own way from the day of birth. One may surmise, then, that in the case of man there is establishment of four additional representations of vocalization in the cerebral cortex, and that each of these four areas preserves an activating connection with the vocalization center in the brain-stem. One may assume also that the appearance of this representation in the cortex is closely related to man's ability to speak.

The new-born kitten or the new-born puppy raises its voice to announce the pangs of hunger, that seem to usher it into the world, just as effectively as the new-born baby does. But, at a later period, when the cerebral cortex begins to function, only the child is ready to learn to speak.

Articulation of words calls for control of movement of the mouth, tongue and throat. These movements have their cortical representation in the lower precentral gyrus of each hemisphere, in the same general area as vocalization. But this cortical representation is found in the other mammals as well as man, no doubt because of the fact that these are the same structures that must be controlled in eating, a function in which man can claim little superiority over his fellow mammals.

Thus, it is clear that the cerebral cortex of man contains within the framework of its voluntary motor system on each side a considerable representation of the movements of articulation and vocalization. This is its inborn organization. It is the motor mechanism for speech, and one hemisphere is as well endowed as the other for this purpose.

But in the learning of speech, which begins at the close of the first year of life, the newly forming ideational mechanism makes its appearance in one hemisphere, not in both. In the elaboration of this function there seems to be a need for a new form of specialization.

It is interesting to observe the positions which the areas devoted to speech come to occupy. The frontal and parietal speech areas

(Fig. 1) are placed on either side of the voluntary sensorimotor strip, quite close to the representation of articulation and vocalization. The supplementary speech area is adjacent to the supplementary area for vocalization. The temporal speech area is placed between the visual cortex and the auditory cortex. This portion of the temporal lobe takes part in essential mechanisms of memory.

Reading and writing seem to depend particularly on the integrity of the cortex between the temporal speech area and the visual area, for injury in this zone results in more severe interference with these functions than with the use and understanding of the spoken word.

Thus, it is apparent that, at birth, mechanisms for movement and sensation are pre-formed. The infant vocalizes and uses its mouth and tongue with great vigor and effectiveness. But the areas which are destined to be utilized for the ideational mechanisms of language and reading and writing are unorganized, waiting for the dawn of understanding.

In the beginning, the child's brain seems to be functionally plastic as far as the location of the speech mechanisms is concerned. If an infant, who has begun to speak, suffers a large injury to the left hemisphere, he may become dumb for as long as a year. Then he learns again by utilization of homologous areas in the cortex of the right hemisphere. An older child might do this but with increasing difficulty.

On the contrary, however, the adult who receives such a widespread injury to the left hemisphere may never succeed in making the transfer, may never speak and read and write again with any degree of perfection. Speech therapy and effort will help repair the defect in inverse ratio to the extent of the lesion. Capacity of substitution of one hemisphere for the other is truly amazing during infancy. But, once functional localization of acquired skills has been established, the early plasticity tends to disappear.

Some of these physiological and anatomical facts that I have discussed in brief outline are new. But many of them have been common knowledge since Broca (1861) first declared that speech had a localized representation in the brain.

It is obvious that the little child, learning to speak his mother's tongue, does so without accent and without apparent effort. When

he learns words he is learning about life. Language provides him with a way of asking for, and usually getting, what he wants; a way to share the exciting ideas, that teem in his new-born imagination, with others who love him; a way of understanding the intriguing romance of fairy tale and the equally intriguing mechanisms of steam engines, dump trucks, animals and dolls. For him language is a means to an end and not an end in itself.

At first he mispronounces his words and he creates a delightful "lingo" of baby talk. But all too soon, perhaps, he drops these mispronunciations and adopts the less interesting way of talking used by the adults about him. The mother's method of teaching language is amazingly efficient!

If, before the age of 10 to 14, the child associates with those who speak a second and even a third language, he can learn by a similar technique two or three languages with no evident increase in his effort.

I had the opportunity of watching this experiment carried out in my own home. Two of our children spoke German in the nursery with a German governess from the beginning. Then, when they first went to school it was to a French nursery school at the ages of 3 and 4. There, work and play were presented to them in the French tongue.

In the nursery with the governess it seemed to them quite natural that the word for dog was "hund." In the nursery school it was "chien" and when they were with their parents it was "dog." There was no confusion, no extra effort, no foreign accents. The change in environment was sufficient to change the way of expression. Or the appearance of the governess who never spoke anything but German might have the same result. The parents also spoke German to the best of their ability in the nursery. This established what may be considered a conditioned reflex.

The experiment was successful. Even the two older children who had played with German children for only a few months at the ages of 9 and 10 continued to talk with the German governess who entered the family at that time. In each case fluent command of the language was achieved without labor, and the ability has since served useful purposes to three of the four children.

Compare that with the experience of their parents who learned English at home in the usual way and later, between the ages of

16 and 22, according to accepted school curricula, they studied French and German with Latin and a smattering of Greek thrown in. Still later Spanish was taken up for a special purpose by the excellent Berlitz method. It was all done with textbooks and word lists and rules of grammar and syntax — home work, toil, trouble and headache.

All this was hard work. The time required was great. The discipline was said to be good for the character. But if the object of it all was to speak easily without accent, to understand quickly and to think in these other languages, the result was certainly a failure.

What happens to the brain when multiple languages are learned? This is an interesting question. Is a separate neurone mechanism for each language set up in different speech areas of the cortex? My answer would be unequivocally " no."

Cases have been reported and published in which injury to the dominant hemisphere has resulted in the loss of one language and the preservation of another. If this claim were substantiated, one would be forced to assume that one area of cortex was used for French and another for Spanish. It is my opinion that careful study of these cases of aphasia would always show involvement of all the languages that have been mastered.

For twenty-five years I have lived and worked in a bilingual society in the Province of Quebec. Many patients speak French and English, often quite fluently. More than once I have heard from an assistant, who was well versed in the published literature on this subject, that an aphasic patient could speak French but was aphasic in English or vice versa. On critical examination it always developed that both languages were involved in the speech defect.

It is characteristic of certain types of aphasia that some words are lost and others retained. Sometimes one or more words are used over and over again. This may be done even when they fail to convey the speaker's meaning. A man may say " yes " when he wants to say " yes " but he continues, to his great chagrin, to say " yes " when he wants to say " no." He has lost the ability to say " no " and he can do no better in French, for he can say neither " oui " nor " non."

When more than one language is learned, the speech areas of the dominant hemisphere take them all on without geographical

separation that one can discover. If languages are learned in the right way, and particularly if they are learned at the right age, multiple languages may be learned perfectly, with little effort and without physiological confusion.

It has been said that an educated Englishman or American need not bother with other tongues. From the usual practical point of view this is no doubt quite true. The little-educated man and the university graduate get along, each with his mother tongue alone, quite well enough under ordinary circumstances. They may have no urge to travel for pleasure, profit or interest. They may not care to read current publications in other languages.

But there are many also who feel the urge to travel and read and who are not prepared for it by the schooling of the present day. There are many who believe that the goal of our present endeavor should be "One World," as Wendell Willkie phrased it. Many believe, as I do, that it is above all the bar of ignorance of language that shuts us off from other peoples, raising as it does a wall about our knowledge and understanding and brotherhood, far more impenetrable than distance or iron curtains.

It has long been one of the avowed objects of education to learn from other cultures. It has been agreed that this could best be done by speaking and reading the language of those other cultures. Hence the standard school exercises in Latin and Greek and in modern language. The problem is a very old one and it was not always solved in the same way.

I spoke of the discovery of writing in Sumer 5000 years and more ago. Semitic conquerors soon came into Sumer speaking a language which we may call Babylonian. They were warlike and untutored. They had much to learn from the more ancient civilization of Sumer. The temple libraries of that land and in that period were filled with clay tablets written in the Sumerian tongue.

Gradually, the common language in city and country came to be Babylonian, but contracts, decisions at law and bills of sale were still invariably written in Sumerian. Educated people understood both tongues.

Thousands of years later, Rome sent out her legions and conquered the world, but Romans had much to learn from Greek culture and their educated people came to know both tongues.

Barbarians in the north of Europe, among them the ancestors of some of us, threw off the domination of Rome. But, through the dark ages that followed, a scattered few, chiefly in the monasteries and castles, kept the Roman language alive and through it ran a slender stream of knowledge from the past until, at last, the day of the Renaissance dawned. Then Roman law, Greek thought and Hebrew religion burst into a new growth and the way was prepared for the birth of something quite new, Modern Science.

Today there linger echoes of this duality of tongue throughout all Christendom, for Latin is still current in the Catholic Church however little it may be understood, and even the relatively illiterate medical man speaks a certain sort of Latin in his anatomy lesson. He also writes Latin in his prescriptions and there, curiously enough, he never fails to bow the knee to an even more ancient culture — for each prescription is prefaced by a sign which resembles an R. It is the sign of Horus and it represents a prayer to that ancient Egyptian god of health that he will remove all evil from the drugs prescribed.

But times have changed and with them the learning of secondary language. The Babylonian once bought a Sumerian slave to teach his children, at an early age, a tongue that would be useful to them when they grew up to become priests, merchants, courtiers. Two thousand years later Roman families of the better class had Greek slaves and much respected Greek tutors.

In more recent times, governesses and maids who spoke foreign tongues could be hired, and this may well be possible in some parts of the world still. But slavery is gone and domestic servants seem to be fast disappearing on this continent.

The time has come to establish new methods of learning the secondary languages. Ignorance of foreign tongues cuts us off from the rest of the world and from the culture of other lands, which is just as important now as it ever was. To make up for this, foreign languages are taught in the final years of school and college. This is good but the effort yields a surprisingly small return.

I know quite well that educators are not in the habit of asking for physiological consultation in the planning of their curricula, and that the neurosurgeon who offers advice, unasked, may well be placing his head in the lion's mouth. This metaphor is par-

ticularly suitable since I am speaking in this venerable Academy of Arts and Sciences, and in Boston where almost everyone is an educator in one field or another.

It may be that the good Lord intended the educated man to read foreign languages painfully and by the sweat of his brow, as a sort of penance, regardless of how easily he learned to manage the mother tongue.

But one who considers only the physiology of the human brain might suggest that a few schools, at least, should be organized somewhat as follows:

Suppose the school which we are to discuss is a day-school in an English-speaking community. Let the first years, from nursery school and kindergarten on to grades for children of eight or ten, be conducted by foreign-born teachers who will speak only their native tongue in school, at work and at organized play.

If such a school is staffed, for example, by German and French teachers, and I make no plea for any particular language, let the little ones begin their years of normal play, drawing, singing and memorizing, in French or German. Teach them no language as such except by the methods so long employed by mother and nursemaid. After, possibly, two years in the French department, let them shift to the German department or vice versa.

Get on from fairy tales to folk literature as rapidly as the child's mind is prepared for it. These children will have been hearing Mother Goose and such things at home, and their play at the week-ends, as well as the home discipline, will have been carried out in English. In their holidays they will, no doubt, speak English.

Then, at the age of eight to ten, let the children take up mathematics, history and all the other subjects that should be mastered, including the dead languages as desired. This later stage may well be conducted in English with occasional exercises in French or German literature and conversation. They will then be able to pronounce their words and to concern themselves with the content of what they read.

During the first stage of education they will be learning to talk and to read and write. If this stage is properly handled the child will come through it with no more effort than when he learns one language instead of two or three.

Surely there are many parents who desire secondary languages for their children now, as in the past. Surely they would support such an undertaking, and the head-master who embarks on such a scheme, utilizing at the same time all that is best in modern pedagogy, will be besieged with applications from far and wide.

I make no pretence to knowledge of the best organization of such a school. My plea is that we should let children hear second-ary languages, properly spoken, at an early age. On this continent we are shut off from easy contact with the rest of the world, and it is necessary to give some thought as to how we may make our-selves good citizens of the world as well as good Americans, good Canadians, good Englishmen.

I realize that there may be exceptional men, capable of learning language quickly at any age. There are always exceptions. It may be that such men, learning a language late in life, can speak with-out accent, although I doubt it. Joseph Conrad, born in Poland, spoke French as a secondary language from infancy. He came to live in England at 20, and became an English author who has thrilled us with his stories of the sea, written in English.

There is a good deal of evidence that he who learns more than one language as a little child has greater facility for the acquisition of additional languages in adult life.

The objection has been made that if a person learns a secondary language quite early, it may be completely forgotten with lack of practice. But let such a person live for a few weeks in the country where that language is spoken and he will discover that his former ability has not been lost nor the skill of pronunciation either.

The duration of man's childhood is very long as compared with that of other animals and the early years are normally devoted to learning a language as a means of learning about life. The human brain has a plasticity at that time and a specialized capacity for acquiring speech which is lost later. Perhaps we who glory in the power of adult reason and who have responsibility for teaching the young, might give some thought to the unalterable evolution of functional specialization in this brain of ours.

When we consider its complexity, composed as it is of thousands of millions of living nerve cells, each sending out or passing on individual electrical currents over insulated wire-like branches, it

does seem an unbelievable, a fabulous structure. It can even be used, as now, for self-contemplation.

The brain of man is the organ of destiny. It holds within its humming mechanisms secrets that will determine the future of the human race. Speech might be called the human brain's first miracle. Speech it was that served to make man what he is, instead of one of the animals.

The brain is a living mechanism, not a machine. In case of breakdown, it can substitute one of its parts for the function of another. But it has its limitations. It is subject to inexorable change with the passage of time.

We might well muse with the Psalmist:

" The days of our years are threescore years and ten; and if by reason of strength they be fourscore years, yet is their strength labour and sorrow; for it is soon cut off, and we fly away." Add, if you choose, another decade or two.

But remember that the brain passes through unalterable transitions. It is especially adapted to the learning of language at one stage and to the use of language in reasoned thought at another, a later stage.

" To everything there is a season and a time to every purpose under heaven." Educators, before all others, must realize that this is particularly true of the " organ of the mind." Physiological evolution causes it to specialize in the learning of language before the ages of 10 to 14. After that gradually, inevitably, it seems to become rigid, slow, less receptive. In regard to this function, it is soon senescent. But it is ready for life's fulfillment in other directions, ready for reasoning, self-discipline, understanding, even wisdom.

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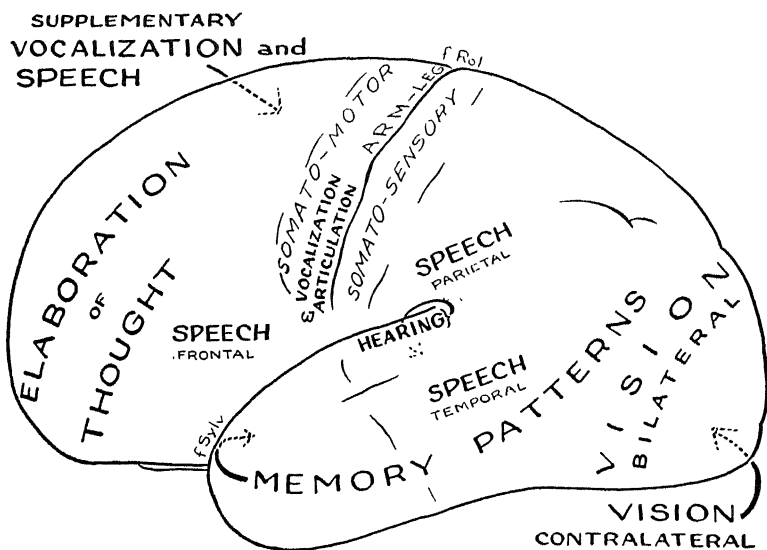
Figure 1

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VOLUME 81

PUBLICATIONS

AMERICAN ACADEMY OF ARTS AND SCIENCES

MONUMENTA PALAEOGRAPHICA VETERA. Series I. Dated Greek Minuscule Manuscripts to the year 1200. Edited by Kirsopp Lake and Silva Lake. 10 fasciculi. 1934-38. $100.00. Separate fasciculi, $12.00 each. Discount to booksellers and Fellows 10%.

Dated Greek Manuscripts. Indices. pp. i-xxv, 1-185. $2.50. The indices are supplied free of charge to subscribers to the entire series.

Series II. An Album of Dated Syriac Manuscripts, by William Henry Paine Hatch. XII and 286 pages, 200 plates. 1946. $25.00. Discount to booksellers and Fellows 20%.

MEMOIRS. Old Series, Vols. 1-4 (1785-1821); New Series, Vols. 1-19 (1833-1946). $10 each. Half volumes, $5 each. Discount to booksellers 25%; to Fellows 50%. Publication of the Memoirs is currently suspended. A few substantially complete sets are still available.

PROCEEDINGS. Vols. 1-81 (1848-1952), $5 each. Discount to booksellers 25%; to Fellows 50%. The individual issues may be obtained separately. A price list of recent issues is printed on the inside of the covers of the Proceedings.

SURVEY OF SLAVIC CIVILIZATION. Vol. 1, December, 1952, Cizevsky, Dmitry: *Outline of Comparative Slavic Literatures.* 143 pp. Paper cover $2.00. Discount to booksellers 25%; to Fellows 50%.

AMERICAN ACADEMY OF ARTS AND SCIENCES

Records of Meetings
1952-1953

VOLUME 82

RECORDS OF MEETINGS
FROM OCTOBER 1952 TO MAY 1953

American Academy of Arts and Sciences

OFFICERS AND COMMITTEES FOR 1952-1953

President
EDWIN H. LAND

Vice-Presidents

Class I
FRANCIS BIRCH

Class II
JAMES M. FAULKNER

Class III
ERWIN D. CANHAM

Class IV
JOHN E. BURCHARD

Secretary
WILLIAM C. GREENE

Treasurer
HORACE S. FORD

Librarian
TAYLOR STARCK

Editor
TAYLOR STARCK

Executive Officer
RALPH W. BURHOE

Councillors

Class I
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Section 2 — PHILIPP FRANK [7]
Section 3 — EUGENE ROCHOW [5]
Section 4 — FRED L. WHIPPLE [4]
Section 5 — MARTIN J. BUERGER [4]
Section 6 — HAROLD L. HAZEN [5]

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Section 1 — A. WARREN STEARNS [4]
Section 2 — RALPH E. FREEMAN [7]
Section 3 — CARL J. FRIEDRICH [5]
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Section 5 — CHESTER M. ALTER [6]

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Section 2 — HUGH O'N. HENCKEN [6]
Section 3 — HOWARD M. JONES [7]
Section 4 — JOSEPH HUDNUT [5]

[4] Term expires May, 1954.
[5] Term expires May, 1955.
[6] Term expires May, 1956.
[7] Term expires May, 1957.

The Rumford Committee
JOSEPH H. KEENAN,[4] *Chairman*

DANIEL F. COMSTOCK [4]

ARTHUR C. HARDY [4]

DONALD F. HORNIG [6]

JOSEPH H. KEENAN [4]

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GEORGE WALD [5]

CUTLER D. WEST [6]

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ROY G. HOSKINS

GREGORY PINCUS

GEORGE W. THORN

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WILBUR K. JORDAN [4]

W. FREEMAN TWADDELL [5]

GEORGE H. EDGELL [6]

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[4] Term expires May, 1954.
[5] Term expires May, 1955.
[6] Term expires May, 1956.

SPECIAL COMMITTEES ACTIVE DURING 1952-1953

Committee for the Conference on Climatic Variations
HARLOW SHAPLEY, *Chairman*

ELSO S. BARGHOORN   HELMUT LANDSBERG
COLUMBUS O'D. ISELIN   HURD C. WILLETT

Committee for the Conference on Totalitarianism
CARL J. FRIEDRICH, *Chairman*

ERWIN D. CANHAM   MERLE FAINSOD
KARL W. DEUTSCH   ALEXANDER GERSCHENKRON
                HAROLD D. LASSWELL

Committee on International Relations
JOHN T. EDSALL, *Chairman*

BART J. BOK   FREDERICK M. ELIOT
CHARLES D. CORYELL   TAYLOR STARCK
PAUL M. DOTY   HENRY R. VIETS

Committee on Policy on Membership and Dues
GEORGE R. HARRISON, *Chairman*

CHESTER M. ALTER   HAROLD L. HAZEN
HORACE S. FORD   HARLOW SHAPLEY
                KENNETH V. THIMANN

Committee on the Rumford Bicentennial
HARLOW SHAPLEY, *Chairman*

PERCY W. BRIDGMAN   JOSEPH H. KEENAN
SANBORN C. BROWN   EDWIN B. WILSON

Committee on School Science
FRANCIS BITTER, *Chairman*

JOHN E. BURCHARD   MASON HAMMOND
CHARLES D. CORYELL   KENNETH V. THIMANN

Committee on Unity of Science
PHILIPP FRANK, *Chairman*

PERCY W. BRIDGMAN   HARLOW SHAPLEY
HUDSON HOAGLAND   B. F. SKINNER
CHARLES MORRIS   S. S. STEVENS

RECORDS OF MEETINGS

October 15, 1952 — Stated Meeting

The One Thousand Three Hundred and Sixtieth Meeting of the Academy convened in its House on October 15, 1952, and was called to order by the President at 8.15 P.M.

There were present ninety Fellows and thirty-seven Guests.

The records of the Meeting of May 14 were approved as published.

The Secretary reported that the Council had approved eight grants-in-aid from the Permanent Science Fund totaling $7,550, and four grants from the Rumford Fund totaling $11,900.

The Secretary announced that the Council had recommended amending the Statutes to remove the requirement that non-resident Fellows pay an admission fee, and recommended that the dues of non-resident Fellows be reduced from $7.50 to $5.00 per year.

Recently elected Fellows were introduced as follows (The Class and Section of the new Fellow, given in parenthesis, is according to the old classification effective at the time of the election.): Donald Leslie Augustine (II:3) by G. C. Shattuck; Pietro Belluschi (IV:4) by J. R. Killian, Jr.; Leo L. Beranek (I:4) by H. L. Hazen; Bruce Hadley Billings (I:2) by D. H. Menzel; Richard Henry Bolt (I:2) by H. L. Hazen; Margaret Clapp (III:4) by H. H. Bundy; Richard Hale Goodwin (II:2) by K. V. Thimann; Lawrence Joseph Heidt (I:3) by J. A. Beattie; James Gordon Horsfall (II:2) by P. C. Mangelsdorf; Henry Garrett Houghton (II:1) by T. K. Sherwood; John Withers Irvine, Jr. (I:3) by I. Amdur; George Wallace Kidder (II:3) by A. S. Romer; Gilbert William King (I:3) by W. H. Stockmayer; Francis Daniels Moore (II:4) by G. W. Thorn; Wayne Buckles Nottingham (I:2) by J. C. Slater; Christopher Hallowell Phillips (III:2) by A. G. Rotch; Burrhus Frederic Skinner (IV:1) by J. G. Beebe-Center; Harry Caesar Solomon (II:4) by R. G. Hoskins; Herbert Henry Uhlig (I:4) by J. Chipman; Hao Wang (IV:1) by W. V. Quine; Hubert Bradford Vickery (II:3) by G. Scatchard; Cornelis Jacobus Gorter (FHM I:2) by J. H. Van Vleck:

The following communication was presented:

Cornelis J. Gorter: *Temperatures Below One Degree Kelvin.*

The meeting was dissolved at 10.30 P.M.

November 12, 1952 — Stated Meeting

The One Thousand Three Hundred and Sixty-First Meeting of the Academy convened in its House on November 12, 1952, and was called to order by the President at 8.20 P.M.

There were present eighty Fellows and thirty-three Guests.

The records of the Meeting of October 15 were approved.

The Secretary reported that the Council this afternoon had recommended amending the Statutes as follows:

Amend Chap. II, Art. 6, par. 1 (1) (see page 149 of the *Proceedings*, Vol. 81, No. 3, January, 1952) to read (in toto): "Every Fellow resident within fifty miles of Boston hereafter elected shall pay an admission fee of ten dollars: if he shall neglect to pay this Fee within six months of the date of his election, his election shall become void. Fellows resident more than fifty miles of Boston shall pay no admission fee."

Amend Chap. II, Art. 6, par. 3, (2) to read (in toto): "Every Fellow residing more than fifty miles from Boston elected after 1938 shall pay, unless he enjoys statutory exemption, such Annual Dues, not exceeding five dollars, as shall be voted annually in March by the Academy. These shall become due on the first of June."

Recently elected Fellows were introduced as follows (The Class and Section of the new Fellow, given in parenthesis, is according to the old classification effective at the time of the election.): Walter Scott Baird (III:4) by O. Oldenberg; John Peterson Elder (IV:3) by W. C. Greene; James Gilluly (II:1) by K. F. Mather; John Anthony Hrones (I:4) by S. C. Collins; Gyorgy Kepes (IV:4) by J. E. Burchard; Morris M. Leighton (II:1) by K. F. Mather; Wolf Leslau (IV:3) by R. H. Pfeiffer; Earl Randall Loew (II:3) by B. R. Lutz; Raymond Cecil Moore (II:1) by K. F. Mather; Thomas Brennan Nolan (II:1) by L. C. Graton; John Alvin Pierce (I:4) by H. R. Mimno; Raphael Salem (II:3) by W. T. Martin; Ascher Herman Shapiro (I:4) by J. H. Keenan; Arnold Max Seligman (I:3) by G. B. Wislocki; Clifford Kenyon Shipton (IV:2) by W. M. Whitehill; Earl Place Stevenson (III:4) by H. S. Ford; Hurd Curtis Willett (II:1) by H. Shapley.

The following communication was presented:
George Wald: *The Molecular Basis of Vision.*
The meeting was dissolved at 10.50 P.M.

December 10, 1952 — Stated Meeting

The One Thousand Three Hundred and Sixty-Second Meeting of the Academy convened in its House on December 10, 1952, and was called to order by the President at 8.20 P.M.

There were present eighty-two Fellows and forty-one Guests.

The records of the Meeting of November 12 were read and approved.

The Secretary reported that the Council this afternoon had recommended raising the limit on the number of Fellows from one thousand to thirteen hundred fifty, and, to accomplish this, to amend the Statutes in Chap. II, Art. 2, par. 1, to read: " The number of Fellows shall not exceed thirteen hundred fifty. . . ."

Recently elected Fellows were introduced as follows (The Class and Section of the new Fellow, given in parenthesis, is according to the old classification effective at the time of the election.): John Otis Brew (III:4) by E. A. Hooton; Halvor Niels Christensen (I:3) by E. G. Ball; I. Bernard Cohen (IV:2) by G. Sarton; Hallam Leonard Movius, Jr. (IV:2) by E. A. Hooton; Gerhard Schmidt (I:3) by D. Rapport; George Davis Snell (II:3) by E. B. Wilson; Frederick John Stare (II:4) by E. G. Ball; Cutler Delong West (I:3) by E. H. Land; Gordon Randolph Willey (IV:2) by E. A. Hooton.

The following communication was presented:

D. M. S. Watson: *Man's Beginnings?*

The meeting was dissolved at 10.05 P.M.

January 14, 1953 — Stated Meeting

The One Thousand Three Hundred and Sixty-Third Meeting of the Academy convened in its House on January 14, 1953, and was called to order by the President at 8.15 P.M.

There were present forty-two Fellows and twenty-five Guests.

The records of the Meeting of December 10 were read and approved.

It was voted to amend the Statutes as follows:

Amend Chap. II, Art. 6, par. 1 to read in toto: " Every Fellow resident within fifty miles of Boston hereafter elected shall pay an admission fee of ten dollars: if he shall neglect to pay this fee within six months of the date of his election, his election shall become void. Fellows resident more than fifty miles of Boston shall pay no admission fee."

Amend Chap. II, Art. 6, par. 3, to read in toto: "Every Fellow residing more than fifty miles from Boston elected after 1938 shall pay, unless he enjoys statutory exemption, such Annual Dues, not exceeding five dollars, as shall be voted annually in March by the Academy. These shall become due on the first of June."

Amend Chap. II, Art. 2, par. 1 to read in part: "The number of Fellows shall not exceed thirteen hundred fifty. . . ."

The following communication was presented:

Arthur E. Sutherland: *The Supreme Court and the General Will: Some Reflections on the Judicial Role in Tomorrow's Democracy.*

The meeting was dissolved at 9.25 P.M.

February 11, 1953 — Stated Meeting

The One Thousand Three Hundred and Sixty-Fourth Meeting of the Academy convened in its House on February 11, 1953, and was called to order by the President at 8.20 P.M.

There were present eighty-one Fellows and eighty Guests.

The records of the Meeting of January 14 were read and approved.

Two Fellows were introduced as follows: Charles R. Morey (IV:4) by W. C. Greene and Harrison Keller (III:5) by W. Piston.

The following communication was presented:

Wilder Penfield: *A Consideration of the Neurophysiological Mechanism of Speech and Some Educational Consequences.*

The meeting was dissolved at 9.45 P.M.

March 6, 1953 — Special Meeting

The One Thousand Three Hundred and Sixty-Fifth Meeting of the Academy convened in its House on March 6, 1953, and was called to order by the President at 8.20 P.M. This Meeting of the Academy was the second of the seven sessions of the Academy's Conference on Totalitarianism.

There were present 270 persons, of whom it is estimated about 110 were Fellows and 160 were Guests.

The following communication was presented:

George F. Kennan: *Totalitarianism and Freedom.*

The meeting was dissolved at 10.15 P.M.

March 26, 1953 — Special Meeting

The One Thousand Three Hundred and Sixty-Sixth Meeting of the Academy convened at the Harvard Club of Boston on March 26, 1953, at 7 P.M. to dine in celebration of the bicentennial of the birth of Benjamin Thompson, Count Rumford. President Land was in the Chair.

This meeting had been designated as Ladies' Night, and there were present ninety-seven Fellows and seventy-six Guests.

The following guests of honor were introduced: Eight former Rumford medalists of the American Academy (Joel Stebbins 1913, Percy Williams Bridgman 1917, Henry Norris Russell 1925, Harlow Shapley 1933, George Russell Harrison 1939, Edwin Herbert Land 1945, Edmund Newton Harvey 1947, and Herbert E. Ives 1951); four former Rumford medalists of the Royal Society (Peter Debye 1930, Karl Manne Georg Siegbahn 1940, Alfred Egerton 1946, and Franz Eugen Simon 1948); two of the three who were to receive the Rumford medals in 1953 (Willis E. Lamb, Jr., and Lars Onsager); the Rumford Professor of Harvard University (Emory Leon Chaffee); and the Representative of the Rumford Historical Society (Richard C. Johnson).

Greetings from the Royal Society and from the Royal Institution of London on the occasion of the celebration of the 200th anniversary of Rumford's birth were presented by Sir Alfred Egerton.

The following communication was presented:

Sanborn Conner Brown: *Count Rumford — Physicist and Technologist.*

The meeting was dissolved at 10.30 P.M.

March 27, 1953 — Special Meeting

The One Thousand Three Hundred and Sixty-Seventh Meeting of the Academy convened in its House on Friday, March 27, 1953, and was called to order by the President at 8.25 P.M. This Meeting of the Academy was the fourth of five sessions devoted to the celebration of the bicentennial of the birth of Benjamin Thompson, Count Rumford, by the Academy, March 26-28.

There were present forty-seven Fellows and forty-three Guests.

Mr. George R. Harrison, Chairman of the Rumford Committee,

presented the following three speakers to introduce the three Rumford medalists of 1953, as follows: Peter Debye, Cornell University, introduced Enrico Fermi, University of Chicago; Norman F. Ramsey, Harvard University, introduced Willis E. Lamb, Jr., Stanford University; and John G. Kirkwood, Yale University, introduced Lars Onsager, Yale University.

Rumford gold and silver medals were then presented as follows: to Enrico Fermi for his studies of radiation theory and nuclear energy, to Willis E. Lamb, Jr., for his studies of the atomic hydrogen spectrum, to Lars Onsager for his contribution to the thermodynamics of transport processes.

The meeting was dissolved at 9.45 P.M., and a reception for the Rumford medalists was held in the Lounge.

May 13, 1953 — Annual Meeting

The One Thousand Three Hundred and Sixty-Eighth Meeting of the Academy convened in its House on May 13, 1953, and was called to order by the President at 8.20 P.M.

There were present fifty-one Fellows.

The records of the Meetings of March 6, 26, and 27 were read and approved.

Annual reports were received as printed below.

Ninety-seven new Fellows, five new Foreign Honorary Members, new officers and committee members were elected as listed below.

Upon recommendation by the Council it was voted that the assessment for the fiscal year beginning April 1, 1953, be $15 for Resident Fellows and $5 for Non-Resident Fellows.

Upon recommendation of the Council it was voted that the following appropriations be made for the fiscal year beginning April 1, 1953:

A. *Appropriations from the General Funds:*

1. Salaries, Pensions, and Annuities	$18,127
2. General Administration	7,800
3. House Maintenance	10,250
4. President's Expense	300
5. Treasurer's Expense	1,000
6. Officers' and Special Committees' Expense	6,000
	$43,477

B. *Anticipated Amounts Available for the Restricted Fund Accounts:*
   1. Amory Fund Committee $25,240
   2. Permanent Science Fund Committee   40,245
   3. Publication Committee   .   11,869
   4. Rumford Fund Committee   28,235
   5. Warren Fund Committee   4,362

Two Fellows were introduced as follows: John K. Galbraith (F III: 2) by C. J. Friedrich; and J. Frederic Dewhurst (F III: 2) by J. K. Galbraith.

Under Mr. Saville R. Davis as moderator, there was in the Lounge beginning at 9 o'clock an informal discussion in search of a healthy relationship between the scholar, his institution, and his government. He was assisted by a panel consisting of Messrs. Martin Deutsch, Erwin Griswold, Norman Levinson and Norman Ramsey.

The meeting was dissolved at 11.30 P.M.

HENRY B. PHILLIPS, *Secretary*

ANNUAL REPORTS

Report of the Secretary on the Status of the Membership
1952-1953

I regret to have to report the following deaths: Twenty-eight Fellows and five Fellows Emeriti — Wilder Dwight Bancroft (I:3), Nathan Banks (II:3), Gregory Paul Baxter (I:3), Frank Weston Benson (IV:4), Francis Gilman Blake (II:4), Giuseppe Antonio Borgese (FE IV:3), Edgar Sheffield Brightman (IV:1), Douglas Houghton Campbell (II:2), Edwin Grant Conklin (II:3), Charles Townsend Copeland (FE IV:4), Harvey Nathaniel Davis (I:2), Gano Dunn (I:6), Gustavus John Esselen (I:3), Wallace Goodrich (IV:4), Herbert Ernest Gregory (II:1), Edgar Erskine Hume (II:4), Edward Vermilye Huntington (II:4), Thomas Augustus Jaggar (I:5), Roland Grubb Kent (IV:3), Arthur Becket Lamb (I:3), Andrew Cowper Lawson (II:1), Warfield Theobald Longcope (II:5), Charles Edward Merriam (III:3), Forest Ray Moulton (I:1), George Rapall Noyes (IV:3), Leigh Page (I:2), William Carter Quinby (FE II:4), Percy Edward Raymond (FE II:1), Hans Reichenbach (IV:1), Michael Ivanovich Rostovtzeff (IV:2), William Thomas Salter (II:4), William Lawrence White (II:2), William Hoyt Worrell (FE IV:3); Seven Foreign Honorary Members — Amado Alonso (IV:3), Gösta Adolfson Bagge (III:5), Benedetto Croce (IV:1), Wilfrid Arthur Greene (III:1), Louis Edouard Lapicque (II:4), Hugh Pattison Macmillan (III:1), Chaim Weizmann (I:3).

Six Fellows have been classed as Emeriti: John Wymond Miller Bunker (II:1), George Raleigh Coffman (IV:3), Hardy Cross (I:6), Gaetano Salvemini (IV:2), Thomas Henry Sanders (III:2), Willard Learoyd Sperry (IV:1).

Nine Fellows have resigned: Barry Bingham (III:4), John Nash Douglas Bush (IV:4), Mark DeWolfe Howe (IV:4), Archibald MacLeish (IV:4), Johnson O'Connor (IV:1), George Parkman (III:1), Jean Joseph Seznec (IV:3), Alfred Henry Sturtevant (II:3), Frits Warmolt Went (II:2).

In May, 1952, eighty-four Fellows and seven Foreign Honorary Members were elected to the Academy; all accepted election.

The roll now includes 953 Fellows, 58 Fellows Emeriti, and 143 Foreign Honorary Members (exclusive of those to be elected tonight).

<div style="text-align: right">
Respectfully submitted,
HENRY B. PHILLIPS, Secretary
</div>

May 13, 1953

Report of the Treasurer

SCHEDULE A

BALANCE SHEET, MARCH 31, 1953

Assets

Cash in Banks and on Hand (Schedule E) ..		$ 20,290.07
Investments:		
General Fund (Estimated Market Value $738,350.25) .	$656,795.81	
Rumford Fund (Estimated Market Value $97,652.75)	86,148.79	
Total Investments (Schedule D) (Estimated Market Value $836,003.00)..		742,944.60
Real Estate, 28 Newbury Street, Boston, Mass. (Book Value)		90,000.00
Prepaid Insurance Premiums		645.89
Total		$853,880.56

Liabilities

Plant Capital		$ 90,000.00
Funds* (Schedule G):		
General:		
Securities	$617,241.66	
Rumford:		
Securities	$86,148.79	
Cash	670.50	86,819.29
Total Funds (Schedule G)		704,060.95
Unexpended Income (Schedule E):		
Securities	$ 40,200.04	
Cash	19,619.57	
Total Unexpended Income		59,819.61
Total		$853,880.56

* Not including principal of Permanent Science Fund, see page 234.

SCHEDULE B

RECEIPTS FOR THE FISCAL YEAR ENDING MARCH 31, 1953*

Academy, General:
Investment Income
 (Schedule D) $37,755.87
 Less: Income to Spe-
  cial Funds 15,148.82

	$ 22,607.05	
House Charges	4,210.00	
Assessments and Admission Fees ...	8,230.00	$ 35,047.05

Mabel S. Agassiz Fund:
Investment Income 120.00
Amory Fund Committee:
Investment Income 4,224.00
Kennelly Fund:
Investment Income 120.00
Life Membership Fund 1,200.00
Permanent Science Fund:
Boston Safe Deposit and Trust Co.,

Trustee	$ 15,000.00	
Return of Grants	665.60	
Investment Income	720.00	16,385.60

Publication Committee:

Investment Income	$ 3,306.00	
Sale of Publications	1,836.23	
Lake Publications	211.78	5,354.01

Publication Exchange Fund:
Linda Hall Library 1,830.34
Rockefeller Foundation Fund:
Institute for the Unity of Science 5,000.00
Rumford Fund Committee:

Investment Income	$ 5,391.02	
Ford Foundation	3,000.00	
Sale of Publications	25.60	8,416.62

Special Endowment Fund:
Investment Income 13.80
Warren Fund Committee:
Investment Income 1,254.00
Total Receipts ... . $ 78,965.42

*Excluding sales of securities.

SCHEDULE C

EXPENDITURES FOR THE FISCAL YEAR ENDING MARCH 31, 1953 *

Academy, General:

Salaries and Pensions, etc.	$ 14,914.65	
Meetings and General Administration	7,383.58	
House Maintenance	9,464.96	
President's Expense	157.68	
Treasurer's Expense	934.31	
Special Appropriations:		
School Science Fund	108.68	
Committee on International Relations	366.71	
Totalitarianism	3,296.97	
Atomic Scientists Bulletin	1,177.69	$ 37,805.23

Mabel S. Agassiz Fund:

For Meetings		120.00

Amory Fund:

Expense		149.48

Institute for the Unity of Science .... | | 1,596.75

Permanent Science Fund:

Grants	$ 12,155.00	
Expense	422.20	12,577.20

Publications Committee:

Bulletin	$ 832.48	
Proceedings	4,533.26	5,365.74

Rumford Fund Committee:

Awards	$ 1,717.20	
Medals	2,158.00	
Bicentennial — Exhibit	1,000.00	
" — General	2,641.11	
" — Foreign Travel	1,953.57	
Climatic Conference	2,877.25	12,347.13

Warren Fund Committee:

Awards		700.00

Total Expenditures .... | | $ 70,661.53

* Excluding purchases of securities.

Schedule D, Investments, *and Schedule E,* Cash, *are not reproduced here, but are available to Fellows upon request of the Treasurer.*

SCHEDULE F

SUMMARY OF OPERATING ACCOUNTS

Account	Balance March 31, 1952	Credits to Account	Expenditures or Transfers	Balance March 31 1953
General	....	$37,805.23	$37,805.23	....
Amory Fund	$16,554.08	4,324.00	149.48	$20,628.60
Lake Publications	1,409.04	211.78	....	1,620.82
Publications	7,279.99	5,142.23	5,365.74	7,056.48
Rockefeller Foundation Fund	873.36	5,000.00	1,596.75	4,276.61
Rumford Fund	27,130.94	8,416.62	12,347.13	23,200.43
Warren Fund	2,482.67	1,254.00	700.00	3,036.67
Totals	$55,730.08	$62,053.86	$57,964.33	$59,819.61

SCHEDULE G – FUNDS

	Balance March 31 1952	Invest- ment Income	Other Fund Receipts*	Fund Expendi- tures*	Transfer for Operating Expenses	Balance March 31, 1953
General Funds:						
Regular	$49,848.38	....	....	....	....	$49,848.38
Life Membership	4,000.00	....	$ 1,200.00	....	....	5,200.00
Current Expense	17,666.05	$22,607.05	....	....	$25,365.23	14,907.87
Barbour Fund	5,000.00	....	....	....	....	5,000.00
Library Sale Fund ..	288,876.91	....	....	....	....	288,876.91
Mabel S. Agassiz Fund .	2,000.00	120.00	....	....	120.00	2,000.00
Amory Fund	53,759.70	4,224.00	....	....	4,224.00	53,759.70
Appleton Fund	17,278.39	....	....	....	....	17,278.39
Centennial Fund	25,608.77	3,306.00	....	....	3,306.00	25,608.77
Kennelly Fund	1,971.08	120.00	....	....	....	2,091.08
Permanent Science Fund						
Income Account	20,692.63	720.00	15,665.60	$12,577.20	....	24,501.03
Publication Exchange						
Fund	11,025.00	....	1,830.34	....	....	12,855.34
Rumford Fund	86,819.29	5,391.02	....	....	5,391.02	86,819.29
Special Endowment Fund	230.95	13.80	....	....	....	244.75
Warren Fund	18,405.16	1,254.00	....	....	1,254.00	18,405.16
Investment Reserve Fund	74,379.36	....	27,752.02	5,467.10	....	96,664.28
Totals	$677,561.67	$37,755.87	$46,447.96	$18,044.30	$39,660.25	$704,060.95

(Schedule B)   (Schedule A)

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*Including gains and losses on sales of securities.

DESCRIPTION OF FUNDS

GENERAL FUNDS

Present Balances $49,848.38 General Fund
5,200.00 Life Membership Fund
14,907.87 Current Expense Fund

Established by various subscriptions and contributions. The income of the first two funds is available for the general purposes and maintenance of the Academy — and both principal and income of the Current Expense Fund are available if needed.

MABEL S. AGASSIZ FUND

Gift of $2,000, 1940. Income for expenses of Academy meetings.

FRANCIS AMORY FUND

Bequest, 1912. Present balance, $53,759.70. For the purpose of establishing a Septennial Prize and a gold medal to encourage the invention and discovery of measures for the relief of maladies peculiar to the bladder and the various organs connected with it. The bequest directed that public notice should be given of the nature and design of the Fund and contributions should be solicited in aid of it — also, that no award of the income be made until twenty-one years after the date of Mr. Amory's death (i.e. 1933). Four awards were made in 1940 and six awards in 1947.

APPLETON FUND

Present balance, $17,278.39. Established in 1854. The Trustees under the will of Samuel Appleton turned over to the Academy certain manufacturing stocks, of the par value of $10,000, for the purpose of constituting "a fund, the income of which is to be applied to the publication of the transactions of the said Society."

THOMAS BARBOUR FUND

Bequest, 1947, $5,000. Unrestricted as to principal and income.

CENTENNIAL PUBLICATION FUND

Present balance, $25,608.77. Established in 1880 by gift of $1,000 from Dr. E. B. Cotting, a Fellow. Balance from subscriptions solicited by the Centennial Committee. Income is applied to publishing the transactions of the Academy.

ARTHUR E. KENNELLY FUND

Bequest, 1939. Present balance, $2,091.08. Unrestricted as to principal and income.

LIBRARY SALE FUND

Sale Price, $320,000. May, 1946. Proceeds for general purposes of the Academy. Present balance, $288,876.91.

PERMANENT SCIENCE FUND

Balance, December 31, 1952, $379,466.14. Fund established in September, 1928, by agreement and declaration of trust of the Boston Safe Deposit and Trust Company, acting as Trustee. Principal of Fund is held and invested by the Trustee and the annual income paid over to the Academy, semi-annually, on request.

PERMANENT SCIENCE FUND (*Income Account*)

Balance, March 31, 1953, $24,501.03. Income of the Permanent Science Fund (above) accumulated and held apart. Available for grants for scientific research on recommendation of the Committee on the Permanent Science Fund of the Academy.

PUBLICATION EXCHANGE FUND

Balance, March 31, 1953, $12,855.34.

RUMFORD FUND

Gift of Count Rumford, 1797, $5,000 in three per cent stock of the United States. Present balance, $86,819.29. Income to be applied every second year as a premium " to the author of the most important discovery or useful improvement which shall be made or published in writing during the preceding two years on heat and light."

In 1852, the Supreme Judicial Court of Massachusetts decreed that the balance of income might be used for the purchase of books, papers, philosophical apparatus, making publications or procuring lecturers, experiments and investigations, thereby carrying out the general intent and purpose of Count Rumford. Because the investment of the Fund could no longer be carried in stock of the United States, investment was permitted in " either notes or debts of the United States, or of the Commonwealth of Massachusetts or of the City of Boston, or in the stock of any bank in this Commonwealth or in notes or bonds secured by pledge of any of said stock or by mortgage of real estate in this Commonwealth or may be deposited in trust, and on interest, with the Massachusetts Hospital Life Insurance Company." This limitation is still in effect.

SPECIAL ENDOWMENT FUND

Gift, 1949, $200. For future development of the Academy. Balance, March 31, 1953, $244.75.

C. M. WARREN FUND

Bequest of Cyrus M. Warren, 1891. Present balance, $18,405.16. The principal is to be held at interest and the income is to be applied toward the encouragement and advancement of research and of science in the field of Chemistry.

Report of the Amory Prize Committee

The Amory Committee met on December 10, 1952, and continued its survey of investigators who should be on its list for final review as of November, 1954. Fourteen new names were considered. The Committee also discussed the fields of research which were appropriate for survey. It was recognized that the control and relief of benign hypertrophy of the prostate was the primary interest of the testator, and that progress in this field would deserve especial consideration, but it was noted that in the absence of adequate progress in the primary field, the prize was available over a reasonably wide area, even including the publication of a treatise in certain lines.

Respectfully submitted,

January 16, 1953   EDWIN B. WILSON, *Chairman*

Report of the House Committee

The Academy met in the House eight times and there were twenty-eight meetings of various committees and conferences under the auspices of the Academy. The average attendance for these thirty-six meetings was sixty-four. A total of twenty-nine other scientific or cultural guest organizations held 154 meetings in the House with an average attendance of seventy-five. It is estimated that the total attendance at meetings and exhibits for the year .was 16,000, or about the same as during the previous year. The table below lists the various guest organizations and the number of meetings held by each.

	No. of Meetings
Alliance Française	7
Alpha and Omega Fraternity, Tufts Dental School	4
Alpha Phi Sorority (Graduates of Salem Teachers College)	2
Boston Authors Club	6
Boston Psychoanalytic Society and Institute	3
Boston Shakespeare Club	1
Boston Society of Civil Engineers	6
Boston Teachers Union	9
Brookline Bird Club	3
Catholic Labor Guild	2
Children's Museum, Trustees	4
China Students' Club	8
Colonial Dames	2
Dynamion Society	14
English Speaking Union	1
Fulbright Scholarship Committee	1
High School Women's Club of Boston	1
Institute of Contemporary Art	5
Japanese Society	1
Joyce Kilmer Post, American Legion, and Auxiliary	25

New England Botanical Club 9
New England Section, Optical Society of America 1
New England School Science Council 12
New England Women's Press Association 6
Planned Parenthood League 1
Polish Literature Club 1
Rhodes Scholarship Committee (Mass. and New England) . 2
Theosophical Society 1
United Cerebral Palsy of Boston . 16

    Total 154

Below are listed the titles of the exhibits in the lobby and the period during which they were displayed. It is estimated that about 1,887 entered the House solely to view the exhibits.

Development of the Back Bay, October 11 to February 1
Early Boston Waterfront, February 1 to March 21
Rumford Bicentennial Exhibit, March 21 to May 13
Parker Medal Exhibit, April 27 to May 12

The costs of maintaining and operating the House are given in the table below:

Expenditures	*1951-1952*	*1952-1953*
Custodian, Assistance, Pensions	$ 3,753.93	$ 4,063.23
Coal	1,202.60	1,617.60
Gas	3.10	0.
Electricity	1,377.21	1,284.61
Elevator	1,489.50	505.02
Water	25.50	21.00
Equipment	618.42	19.70
Supplies	428.30	747.53
Alterations and repairs	757.25	1,147.01
Fire Insurance	520.05	578.75
Miscellaneous	67.84	80.23
Total	$10,243.70	$10,064.68
Appropriations (including pension)	11,875.00	10,650.00
Unexpended	$ 1,631.30	$ 585.32

Receipts for use of the facilities of the House by other scientific, educational, and cultural organizations reduced the net cost to the Academy as follows:

	1951-1952	*1952-1953*
Total operating expenditures	$10,243.70	$10,064.68
Actual receipts for the year	4,257.85	4,210.00
Net cost of House Maintenance and Operations to the Academy	$ 5,985.85	$ 5,854.68

The Committee on International Scientific Publication of the National Research Council continued to occupy office space on the fourth floor during the year. On January 1 they moved from the smaller to the larger office space. The smaller office (570 square feet) is now available for other occupancy. The Boston Museum of Science and the Boston Authors Club continued to use a share of the space in the book-stack wing, and the Boston Medical Library began to occupy a section of this beginning January 1.

Respectfully submitted,

May 13, 1953   JOHN B. WILBUR, *Chairman*

Report of the Permanent Science Fund Committee

At our October meeting we had thirteen applications to consider, totaling requests for $14,757, of which eight were acted upon favorably involving awards of $7,550. Four of the applications were declined.

In March, twenty applications requesting a total of $21,953 were presented for consideration. The 1952-1953 income of the Fund was $16,385.60 but, with our reserve, $23,828.08 was available as of March 10, 1953. Of the twenty applications, seven were declined; and thirteen were approved, for a total amount of $9,565.

Grants were approved by the Council of the Academy on October 15, 1952, and on April 8, 1953, as follows:

Grants Approved October 15, 1952

1. To Nathan Belfer, Assistant Professor of Economics, Department of Economics, Pennsylvania State College, State College, Pa., for studies of the economic and social significance of the new steel plants being constructed at Morrisville, Pa., $1,000.

2. To Leon S. Ciereszko, Associate Professor of Chemistry, University of Oklahoma, Box 96, Faculty Exchange, Norman, Okla., for a study of the comparative biochemistry of Echinoderms and Tunicates, $950.

3. To Ralph I. Dorfman, Associate Director of Laboratories, Worcester Foundation for Experimental Biology, 222 Maple Avenue, Shrewsbury, Mass., for a study of the infrared analysis of steroids, $1,500.

4. To Benjamin Karpman, Chief Psychotherapist, Saint Elizabeths Hospital, Washington, D. C., for work on an outline for the study of juvenile delinquency to include infant and child psychopathy and delinquency, $750.

5. To O. H. Robertson, Lecturer, Dept. of Biological Sciences, Stanford University, Stanford, Calif., for a study of the cause of death of the Pacific salmons after spawning, $1,200.

6. To Thomas W. Whitaker, Senior Geneticist, U. S. D. A., P. O. Box 150, La Jolla, Calif., for a study of the cultivated *Cucurbitaceae,* their origin, distribution, archeology, and genetics, $1,000.

7. To the Massachusetts Historical Society, 1154 Boylston Street, Boston, Mass., to aid in the preparation for publication of the private and public papers of President John Adams, President John Quincy Adams, and Charles Francis Adams (1807-1886), $1,000.

8. To D. V. Widder, Professor of Mathematics, Harvard University, Cambridge, Mass., for preparing for publication of a book entitled "The Convolution Transform," $150.

Grants Approved April 8, 1953

1. To Philip F. Bonhag, Assistant Professor of Entomology, Iowa State College, Ames, Iowa, for nucleic acid studies of the ovarian trophocytes of *Oncopeltus fasciatus*, $500.

2. To Elisabeth Deichmann, Curator of Marine Invertebrates, Museum of Comparative Zoology, Oxford Street, Cambridge 38, Massachusetts, for a report on the Holothurians of the Antarctic region on the basis of the "Discovery" expeditions material, $250.

3. To Robert J. Drake, Curator, Laboratory of Conchology, Biblioteca y Museo de Sonora, Hermosillo, Sonora, Mexico, for a study of the continuation of conchological and ethnoconchological research in West Mexico, $300.

4. To Robert L. Edwards, Instructor in Biology, and J. Lockwood Chamberlin, Teaching Fellow in Biology, Brandeis University, Waltham, Massachusetts, for selected studies of the ecology and Pleistocene geology of Richmond Gulf, Hudson Bay, Canada, $600.

5. To Alexander Forbes, Professor of Physiology, Emeritus, Harvard University, Biological Laboratories, Divinity Avenue, Cambridge, Massachusetts, for a study of electric responses of vertebrate retina to color shift, $1,000.

6. To Cecilia Payne-Gaposchkin, Astronomer, and Bart J. Bok, Professor of Astronomy, Harvard College Observatory, Cambridge 38, Massachusetts, toward purchasing an Eichner variable-aperture photometer, $1,000.

7. To Frederick G. Hofmann, Research Fellow in Endocrinology, Harvard School of Dental Medicine, 188 Longwood Avenue, Boston, Massachusetts, for a study of the sulfur metabolism of the adrenal cortex in the rat, $500.

8. To R. S. MacNeish, Anthropologist, National Museum of Canada, Ottawa, Ontario, Canada, for the development of maize agriculture and civilization in prehistoric Meso-America, $2,000.

9. To Loren D. Potter, Associate Professor, North Dakota Agricultural College, Botany Department, College Station, Fargo, North Dakota, for a study of postglacial climatology and forest succession of Northwestern Ontario, $1,200.

10. To George R. Rumney, Assistant Professor of Geography, University of Connecticut, Storrs, Connecticut, for a study of limits of settlement on the Canadian shield, $1,000.

11. To Charles G. Sibley, Assistant Professor of Zoology, San Jose State College, San Jose 14, California, for a study of introgressive hybridization in the Red-eyed Towhee, $640.

12. To Bradford Washburn, Director of the Museum of Science, Science Park, Boston 14, Massachusetts, to develop public interest and knowledge of the science of Astronomy, its materials, its tools and its methods, and to investigate effective procedures in this field of science education, $500.

13. To Sidney E. White, Assistant Professor, The Ohio State University, Department of Geology, Columbus 14, Ohio, for a geologic investigation of the late Pleistocene history of the volcano Ixtaccíhuatl, Mexico, $75.

Respectfully submitted,

May 13, 1953   HUDSON HOAGLAND, *Chairman*

Report of the Committee on Publication

The Committee held no formal meetings during the year.

No numbers of the *Memoirs* appeared during the year. Volume 80 of the *Proceedings*, devoted to papers of the Institute for the Unity of Science (see *Proceedings*, Volume 79, page 345 and Volume 81, page 316), is still incomplete. Number 2, pages 113-186, dated May, 1952, was distributed in August. Number 3, pages 187-270, dated March, will be mailed this month (see *Bulletin*, Volume VI, Number 8). Copy for the fourth number has not yet been received. Volume 81 of the *Proceedings*, 327 pages, was completed with the publication of the seventh number, Records of Meetings, in July, 1952. Volume 82 was begun with the Winter Number in March, 1953, and will be complete with the publication of the Rumford Bicentennial number this summer. This last number will be sent to all Fellows of the Academy, as well as Numbers 3, 4, and 5 containing the Communications presented to the Academy at regular meetings by C. J. Gorter, Arthur E. Sutherland, and Wilder Penfield, and Number 6, Records of Meetings. Number 2, about to be distributed, is devoted to three research papers by P. W. Bridgman.

The *Bulletin* was issued eight times under the direction of Mr. Burhoe. It contained abstracts of the Communications by George Wald and D. M. S. Watson, in addition to announcements and current news.

The proceedings of the Symposium on Climatic Change, held in May, 1952, are being published as a book by the Harvard University Press. It is expected before Christmas. Fellows may order a copy at a fifty per cent discount.

The Conference on Totalitarianism was held by the Academy in March, 1953. The papers presented at this conference will likewise be published in book form by the Harvard University Press in the fall. Fellows may order one copy of this book through the Academy office at a discount of fifty per cent.

The "Survey of Slavic Civilization" (see *Proceedings*, Volume 81, page 316) was inaugurated with the *Outline of Comparative Slavic Literatures* by Dmitry Cizevsky, published in January, 1953. Copy for the second volume has just been received. This number, *The Slavs: Their Early History and Civilization* by Francis Dvornik, should appear in the fall. Other titles of the projected series were listed in the February *Bulletin*.

The Committee on Publication received during the year from investment income $3,306.00, from sales of publications $2,048.01. Of the sales, $338.78 is credited to special accounts. With the balance on hand, April 1, 1952, of $8,689.03 the Committee had at its disposal a total of $14,043.04. The expenditures to March 31, 1953, were for *Bulletin* and *Proceedings* $5,365.74, leaving a balance on April first of the present year of $8,677.30. With estimated investment income of $3,192 the Committee has at its disposal $11,869.30 for 1953-54. Sales will amount to about $2,000 additional. This accounting appears more favorable than will be the case when all bills have been received. These will reduce the balance by more than $3,000.

In last year's report the Chairman estimated that the Committee's publication reserves would be exhausted by March 31, 1954, if the rate of publication then in effect were continued. It appears now that we have another year of grace. Investment income and sales are not sufficient to cover the cost of a 300-page volume of *Proceedings* and the *Bulletin*. Other publications, and some of the *Proceedings*, have been and must continue to be financed in whole or in part by special subventions or gifts. It has been generally agreed in informal discussions that the publication program of the Academy cannot be further curtailed. When the reserve fund is finally exhausted, the inevitable annual deficit in the accounts of the Committee on Publication must be covered by general funds of the Academy.

Respectfully submitted,

May 13, 1953   TAYLOR STARCK, *Chairman*

Report of the Rumford Committee

The dominating activity of the Rumford Committee during the past year has been the celebration of the bicentennial of the birth of Benjamin Thompson, Count Rumford, which took place at Woburn, Massachusetts, March 26, 1753. The celebration took place on Thursday, Friday, and Saturday, March 26, 27, and 28, 1953. The Bicentennial Birthday Banquet, held at the Harvard Club of Boston on the evening of March 26 and attended by 97 Fellows and their 76 ladies, commemorated the Count not only through Professor Sanborn C. Brown's account of his life and work, but also through the menu which was selected from some of Rumford's own writing on the subject.

On the second evening of the celebration, three awards of the Rumford Premium were made: to Enrico Fermi of Chicago University for his studies of radiation theory and nuclear energy, to Willis Eugene Lamb, Jr., of Stanford University for his studies of the atomic hydrogen spectrum, and to Lars Onsager of Yale University for his contribution to the thermodynamics of transport processes. This is the first occasion when more than one Premium has been presented.

The third feature of the celebration was a series of three symposia held on March 27 and 28 on recent developments in (1) thermodynamics, (2) atomic spectroscopy, and (3) nuclear physics. The papers of the bicentennial are to be published in a special issue of the *Proceedings* of the Academy. An exhibition of the scientific works of Count Rumford together with portraits and other Rumfordiana was prepared by Professor Sanborn C. Brown of M. I. T. for display particularly during the bicentennial year. Guests of honor at the celebration included nine former medalists and four Rumford medalists of the Royal Society. Most of the arrangements for the bicentennial celebration were made by a subcommittee under the chairmanship of Harlow Shapley. Serving on the subcommittee also were P. W. Bridgman, Sanborn C. Brown, Joseph H. Keenan, and Edwin B. Wilson.

By mail vote of the Committee on June 10, a grant of $800 was recommended for Professor Stanley S. Ballard, Physics Department, Tufts College,

Medford, Mass., in support of the design and construction of an apparatus for measuring the thermal expansion of small samples of optical crystals. This was approved by the Council at its October meeting.

By mail vote of the Committee on July 21, a grant of $700 was recommended to Dr. Peter Elias, Harvard University, working at the Polaroid Corporation, Cambridge, Mass., for studies relating to the formation and interpretation of images formed by optical systems. This was approved by the Council in October.

The Committee at its meeting on October 14 recommended an appropriation of $4,000 to the Harvard University Press to underwrite the publication of a book by the Press covering the papers presented at the Academy's Rumford-fund-supported symposium on climatic change; and an appropriation of $6,400 for expenses of the bicentennial celebration. Both of these were approved by the Council at its October meeting.

By mail vote of the Committee on February 16, a grant of $500 to Professor Forrest F. Cleveland, Department of Physics, Illinois Institute of Technology, Chicago 16, Ill., was recommended for his program on Raman and infrared spectral data, assignments, force constants, and thermodynamic properties of substituted methanes and ethanes. The Council approved this at its meeting in April.

By mail vote of the Committee on April 6, a grant of $500 was recommended to Dr. Theodore Dunham, Jr., Institute of Optics, University of Rochester, Rochester 3, N. Y., to cover part of his travel to Canberra, Australia, to organize further a cooperative program between Australian and American astronomers for photographing with high dispersion the spectra of the stars of the Southern Hemisphere. This was approved by the Council at its May meeting.

Respectfully submitted,

May 13, 1953   GEORGE R. HARRISON, *Chairman*

Report of the Cyrus M. Warren Committee

The Committee had available for grants the sum of $3,000, which amount had been voted by the Council at the meeting on March 12, 1952; representing $1,500 voted for the preceding year but not used and $1,500 voted from present funds.

At a meeting of the Committee held on May 12, 1952, the following grants were voted:

To Professor L. S. Ciereszko of the University of Oklahoma, a grant of $200 for the purchase of special supplies for research on the application of urea adduct (or complex) formation to the separation of fatty alcohols and their glyceryl ethers.

To Dr. Lillian N. Ellis and Mr. Sheldon Atlas of Adelphi College, Garden City, N. Y., a grant of $500 to be used for component parts of an optical system for light-scattering measurements.

At the meeting of the Council on May 13, 1953, the following projects recommended by the Committee were approved:

To W. Lewis Nobles, Assistant Professor of Pharmaceutical Chemistry, University of Mississippi, University, Mississippi, for studies of the application of the Mannich reaction to a, β-unsaturated ketones. ($225.00)

To Walter R. Hearn, Instructor in Biochemistry, Baylor University College of Medicine, Houston, Texas, for preparation and study of tyrosine-containing peptides and peptide derivatives. ($300.00)

A letter was received from Professor A. J. Castro, San Jose State College, San Jose, California, submitting a report of the expenditure of the grant of $55.00 in 1951. The apparatus purchased by Professor Castro, a Powerstat and Stirrer, has been returned to the Chairman for use by others.

A report from Dr. Lillian N. Ellis indicates that progress has been made in assembling the equipment required, and the plan is to spend the coming summer and perhaps the following summer in completing the research proposed.

A report has been received from Professor L. S. Ciereszko and he states that the project he started with these funds is to be continued under a grant from the National Institute of Health.

A reprint has been received from E. R. Atkinson covering work carried out under a grant from the Cyrus M. Warren Fund: "A Polarographic Examination of Diazotized Amines. II Additional Coulometric Data" by E. R. Atkinson, Charles E. Garland and Arthur F. Butler. *J. Am. Chem. Soc.* 75, 983 (1950).

Respectfully submitted,

May 13, 1953   E. R. GILLILAND, *Chairman*

OFFICERS AND COMMITTEES ELECTED MAY 13, 1953

For a complete list of officers and committees see the first pages of this issue.

President . . . . . . Edwin H. Land
Vice-President of Class I . . Francis Birch
Vice-President of Class II . . James M. Faulkner
Vice-President of Class III . . Erwin D. Canham
Vice-President of Class IV . . John E. Burchard
Secretary . . . . . . William C. Greene
Treasurer . . . . . . Horace S. Ford
Librarian . . . . . . Taylor Starck
Editor . . . . Taylor Starck

Councillors
To serve for four years:

Garrett Birkhoff, *of Class I, Section 1*
Philipp Frank, *of Class I, Section 2*
Carroll M. Williams, *of Class II, Section 4*
Ralph E. Freeman, *of Class III, Section 2*
Howard M. Jones, *of Class IV, Section 3*

House Committee

Philippe E. LeCorbeiller .   *of the Committee, to serve as Chairman*
Thomas K. Sherwood   . . . . *to serve for three years*

Committee on Membership
To serve for two years:

Bart J. Bok, *of Class I*   Henry A. Murray, Jr., *of Class III*
Francis O. Schmitt, *of Class II*   William G. Constable, *of Class IV*

Committee on Meetings
To serve for two years:

Samuel A. Stouffer, *of Class III*   Esther Forbes, *of Class IV*

Finance Committee
To serve for two years:

Harvey H. Bundy   Erwin H. Schell

Auditing Committee

David F. Edwards . .   .   .   *to serve for two years*

Committee on Publication
To serve for two years:

Thomas J. Wilson, *of Class III*   Waldo G. Leland, *of Class IV*

Permanent Science Fund Committee

J. Lawrence Oncley . . . . *of the Committee, to serve as Chairman*
Earl R. Loew .   . . .   '.   . . *to serve for three years*
Eric G. Ball   . . . .   *to serve for three years*

Rumford Committee

Joseph H. Keenan . . . . *of the Committee, to serve as Chairman*
Donald F. Hornig . . . . *to serve for three years*
Cutler D. West   . . . . *to serve for three years*

Warren Committee
To serve for three years:

Arthur C. Cope   Richard C. Lord

Amory Prize Committee
To serve for one year:
Edwin B. Wilson, *Chairman*

William B. Castle   .   George W. Thorn
Roy G. Hoskins   George B. Wislocki
Gregory Pincus   S. Burt Wolbach

Nominating Committee
To serve for three years:

William P. Allis, *of Class I*   Saville R. Davis, *of Class III*
L. R. Cleveland, *of Class II*   George H. Edgell, *of Class IV*

NEW MEMBERS ELECTED MAY 13, 1953

FELLOWS (97)

CLASS I — MATHEMATICAL AND PHYSICAL SCIENCES (25)

Section 1 — Mathematics (1)

George Whitelaw Mackey   Harvard University, Cambridge, Mass.

Section 2 — Physics (6)

Robert Fox Bacher   California Institute of Technology, Pasadena, Calif.
Sanborn Conner Brown   Mass. Institute of Technology, Cambridge, Mass.
Martin Deutsch   Mass. Institute of Technology, Cambridge, Mass.
Brian O'Brien   University of Rochester, Rochester, N. Y.
William Shockley   Bell Telephone Laboratories, Murray Hill, N. J.
Julian Hale Webb   Eastman Kodak Company, Rochester, N. Y.

Section 3 — Chemistry (5)

Howard Oldford McMahon   Arthur D. Little, Inc., Cambridge, Mass.
Eugene Rabinowitch   University of Illinois, Urbana, Ill.
Erwin Schwenk
        Worcester Foundation for Experimental Biology, Shrewsbury, Mass.
Charles Gardner Swain   Mass. Institute of Technology, Cambridge, Mass.
Arthur Andrew Vernon   Northeastern University, Boston, Mass.

Section 4 — Astronomy (3)

William Wilson Morgan   Yerkes Observatory, Williams Bay, Wisc.
Jason John Nassau   Warner and Swasey Observatory, E. Cleveland, Ohio
Lyman Spitzer   Princeton University, Princeton, N. J.

Section 5 — Earth Sciences (3)

Chester Ray Longwell   Yale University, New Haven, Conn.
William Walden Rubey   National Research Council, Washington, D. C.
Harry Blackmore Whittington   Harvard University, Cambridge, Mass.

Section 6 — Engineering Sciences and Technologies (7)

George Francis Carrier   Harvard University, Cambridge, Mass.
Albert Gordon Hill   Mass. Institute of Technology, Cambridge, Mass.
William MacGregor Murray
                    Mass. Institute of Technology, Cambridge, Mass.

CLASS I — *Section 6* — Continued

Milton Clayton Shaw   Mass. Institute of Technology, Cambridge, Mass.
Percy LeBaron Spencer Raytheon Manufacturing Company, Waltham, Mass.
Horton Guyford Stever Mass. Institute of Technology, Cambridge, Mass.
Jerome Bert Wiesner   Mass. Institute of Technology, Cambridge, Mass.

CLASS II — BIOLOGICAL SCIENCES (26)

Section 1 — Biophysics and Biochemistry (8)

John Machlin Buchañan   Mass. Institute of Technology, Cambridge, Mass.
Gerty Theresa Cori   Washington University, St. Louis, Mo.
Ralph Isadore Dorfman
          Worcester Foundation for Experimental Biology, Shrewsbury, Mass.
Conrad Arnold Elvehjem   Wisconsin University, Madison, Wisc.
Robert Emerson   University of Illinois, Urbana, Illinois
Joseph Stewart Fruton   Yale University, New Haven, Conn.
Walter Lee Hughes, Jr.   Harvard University, Cambridge, Mass.
Barbara Wharton Low (Mrs. Metchie Budka)
                         Harvard University, Cambridge, Mass.

Section 2 — Botany and Bacteriology (4)

Henry Ives Baldwin   Fox Research Forest, Hillsboro, N. H.
Richard Alden Howard   University of Connecticut, Storrs, Conn.
Ernest Robert Sears   University of Missouri, Columbia, Mo.
Frans Verdoorn   Chronica Botanica, Waltham, Mass.

Section 3 — Zoology (5)

Philip Jackson Darlington   Harvard University, Cambridge, Mass.
Theodosius Dobzhansky   Columbia University, New York, N. Y.
Tillie Edinger   Harvard University, Cambridge, Mass.
Walter Landauer   University of Connecticut, Storrs, Conn.
Franz Schrader   Columbia University, New York, N. Y.

Section 4 — Physiology and Experimental Psychology (4)

Frank Ambrose Beach   Yale University, New Haven, Conn.
Joseph Carl Robnett Licklider
              Mass. Institute of Technology, Cambridge, Mass.
Ross Armstrong McFarland Harvard School of Public Health, Boston, Mass.
Edwin Broomell Newman   Harvard University, Cambridge, Mass.

Section 5 — Medicine (5)

Derek Ernest Denny-Brown   Harvard University, Cambridge, Mass.
Jack Ewalt   Mass. Department of Mental Health, Boston, Mass.
Jacob Fine   Harvard University, Cambridge, Mass.
Joseph Garland   New England Journal of Medicine, Boston, Mass.
Joseph Foster Ross   Boston University, Boston, Mass.

CLASS III — SOCIAL ARTS AND SCIENCES (20)

Section 1 — Social Relations (7)

John William Gardner	Carnegie Institution, New York, N. Y.
Francis Keppel	Harvard University, Cambridge, Mass.
Douglas Llewellyn Oliver	Harvard University, Cambridge, Mass.
Frederick Osborn	New York, N. Y.
Leslie Spier	University of New Mexico, Albuquerque, New Mexico
Fletcher Guard Watson	Harvard University, Cambridge, Mass.
Frederick Roelker Wulsin	Tufts College, Medford, Mass.

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Boris Mirkine-Guetzevitch,	École Libre des Hautes Études, New York, N. Y.

Section 4 — Law (1)

Peter Woodbury	U. S. Court of Appeals, Manchester, N. H.

Section 5 — Administration and Affairs (10)

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Thomas Boylston Adams	Sheraton Corporation of America, Boston, Mass.
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Sevellon Ledyard Brown	Providence Journal, Providence, R. I.
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Edward Sanborn French	Boston & Maine Railroad, Springfield, Vt.
William Gurdon Saltonstall	Phillips Exeter Academy, Exeter, N. H.
Adlai Ewing Stevenson	Springfield, Illinois
Vernon Dale Tate	Mass. Institute of Technology, Cambridge, Mass.

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George Hunston Williams	Harvard Theological School, Cambridge, Mass.
John Joseph Wright, Bishop of Worcester	Worcester, Mass.

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Henry Russell Hitchcock	Smith College, Northampton, Mass.
Alfred Vincent Kidder	Carnegie Institution, New York, N. Y.
William Leonard Langer	Harvard University, Cambridge, Mass.
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Bartlett Jere Whiting	Harvard University, Cambridge, Mass.

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Section 2 — *Botany and Bacteriology* (1)

Albert Jan Kluyver	Technical University, Delft, Holland

Section 3 — *Zoology* (1)

David Meredith Seares Watson	University College, London, England

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Section 3 — *Political Science* (2)

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2. FRANK, PHILIPP — The Origin of the Separation between Science and Philosophy. BARBER, BERNARD, and MERTON, ROBERT K. — Brief Bibliography for the Sociology of Science. CHURCH, ALONZO — Brief Bibliography of Formal Logic. THOMPSON, LAURA — Some Significant Trends toward Integration in the Sciences of Man. pp. 113-186. May, 1952. $1.45.

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THE

RUMFORD BICENTENNIAL

A record of scientific and biographical papers
presented at the symposium marking the two-
hundredth anniversary of the birth of Benjamin
Thompson, Count Rumford, held at the American
Academy of Arts and Sciences in Boston on the
twenty-sixth to the twenty-eighth of March, 1953.

edited by

HARLOW SHAPLEY

Chairman of the Committee on the Rumford Bicentennial
American Academy of Arts and Sciences

CONTENTS

The Rumford Bicentennial

HARLOW SHAPLEY
Harvard University

There is no doubt about it, and it is not a savory thought that although he lived at the time of the French and American wars against tyranny, Count Rumford was instinctively a totalitarian. He was a believer in the ordered state, in the planned and efficient economy, in the detailed regulation of the common life. And while his genius was largely turned toward alleviating the discomforts of the poor and toward the enhancement of the general standard of living, it appears that he thoroughly disliked most of his fellow men, especially those of high estate in politics and science. He was in fact a multiple personality, richly equipped with contrasting virtues and vices — mostly virtues. He was a scientific genius, intermittent scoundrel, philanthropist, military wizard, practical inventor, gourmet and dietitian, international spy, architect, intellectual pioneer, ingenious cook, soldier of fortune, and one of the greatest practical servants of mankind — altogether an attractive character. If he had had the genial personality and love of his fellows exhibited by Benjamin Franklin, or the broad humanity of Leonardo da Vinci, or the sympathetic insight into human nature of Goethe, he would have ranked with them among the world's great. But such softening qualities might well have sterilized his restive genius and certainly would have prevented this poor apprentice boy-clerk out of a Boston suburb from becoming one of the near great successively in Bavaria, England, and France.

For his less commendable characteristics Rumford was chased out of New Hampshire and Massachusetts by indignant fellow citizens. He was a traitor to his own country and presumably later a traitor to England, which he twice left while under clouds of suspicion and calumny. Although he was knighted by England and by Bavaria (the Holy Roman Empire), both countries were glad to be rid of him. France liked him little for his quarrelsomeness and opportunistic operations, but greatly admired his science and technology. These and other picturesque features of Rumford's useful and unhappy life are presented in the following

pages in a charming sketch by his biographer, Professor Sanborn C. Brown.

The versatile Count's political chicanery and his marital and extramarital activities, so far as they are known and are of significance in the proper interpretation of his life and social philosophy, will be reported more fully in Brown's forthcoming full-length biography. It is my surmise that Rumford's asset to society far exceeded his detriment. It is a grim compliment that probably only the high scientific skill with which Rumford wrote his traitorous invisible-ink report to the British about the equipment of our embattled Revolutionary forebears saved him from hanging in his early twenties, many years before he made his revolutionary experiments on heat and light, and introduced lasting social reforms. But we won the War of the Revolution and should, or at least can, forgive him, wondering meanwhile if other potential scientific benefactors have had less good fortune on the fringes of the law. The great Lavoisier lost his head to the French guillotine, but that paid off in a way, since his rich widow married the dashing Count Rumford and thus helped to finance his last prolific years of brilliant scientific research.

The diverse scientific and social interests of Count Rumford, as well as his personal traits, make him a most satisfactory subject for bicentennial commemoration. The American Academy of Arts and Sciences, which owes much to the famous expatriate because of his establishment of its Rumford Premium which has honored and encouraged American scientists for more than a century, appropriately undertook a celebration. It was held on March 26, 1953, the two-hundredth anniversary of Count Rumford's birth, and on the two days following. As chairman of the bicentennial committee, I shall undertake to report here on the celebration and provide a brief introduction to the program of scientific discussions. The papers presented at the three technical sessions, or summaries of them, appear on the following pages.

Heat and light were the subjects that deeply interested Count Rumford, in addition to shady politics, militarism, and social planning and doing. In these days of great emphasis on electronics, nuclear energy, and related exciting phases of physico-chemical

science, relatively little interest is shown in the properties of heat. It is mostly a subject for engineers, not for academic scientists. A century and a half ago, however, the subject was of much theoretical and practical import. Atomic physics, spectroscopy, physiology, biological evolution, and most of the rampant modern scientific technologies had not been born. The Caloric theory, the most famous of false doctrines (after astrology and similar black arts), ruled the physical sciences and scientific philosophy. Rumford was suspicious of the Caloric theory. From suspicion he proceeded to investigations — from investigation to conviction that the hypothesis is false, although it took decades for the full acceptance of the evidence he produced and for the writing of the obituary of Caloric. His experiments and arguments were basic in slaying the persisting hypothesis that heat is a fluid without substance. The dogma was long and ingeniously defended.

The association of heat and light were as natural in Rumford's day as now. The original conditions for the award of the Rumford Premium by the American Academy of Arts and Sciences emphasize the union. They read in part as follows:

> ... that the interest [on the endowment of five thousand dollars] ... may be ... given once every second year, as a premium, to the author of the most important discovery or useful improvement, which shall be made and published by printing, or in any way made known to the public, in any part of the Continent of America, or in any of the American Islands during the preceding two years, on Heat, or on Light. ... If during any term of two years, reckoning from the last adjudication . . . no new discovery or improvements should be made in any part of America relative to either of the subjects in question (Heat or Light), which, in the opinion of the Academy, shall be of sufficient importance to deserve this Premium, in that case it is my desire that this Premium may not be given, but that the value of it may be reserved, and by laying out in the purchase of additional stock in the American funds may be applied to augment the capital of this Premium.

The committees on the Rumford Award have modified the stipulations from time to time, sometimes with the help of appropriate legal decisions. For example, in practice the concept

" radiation " has been substituted for " light," and we have then reached out beyond ordinary visual light in both directions – through the red all the way to radio waves, and through the violet down to gamma radiation, even to cosmic rays. Also it was found long ago that the awarding of medals could not be justified often enough to prevent the income from the original endowment from increasing toward a sum impractical as an endowment for medals, even when the medals are large and made of both gold and silver. In consequence, with permission of the Court, the accumulated income has been for about one hundred years used on nearly four hundred occasions for grants in aid of research in the broad fields of heat and light. These grants have been used to pay for material, instruments, technical assistance, travel, and other expenses of the investigations. This aid to American science has been important over the past century, but its relative value has recently diminished because of the creation of other granting agencies and the growth of very elaborate assistance to research in heat and light from governmental sources. The number of applications to the Rumford fund has fallen off, and the accumulated income has from time to time been added to capital.

Harvard College, the beneficiary of Rumford's residuary estate under his will executed in France in 1812, made an effort in 1832 to commandeer the accrued interest of the Academy's Rumford fund. Judge Shaw of the Supreme Court of the Commonwealth of Massachusetts considered the evidence fully, noted the revised plan of the Academy for the use of the Rumford fund, and said no. He said it emphatically. And the fund that was originally five thousand dollars is now approximately one hundred thousand dollars.

By 1953 the growth of the Rumford fund and of its income had provided the agreeable circumstance that the gift made by Count Rumford in 1796 was able to carry most of the expense of the celebration of his own bicentennial. Such a practical husbanding of physical resources, as well as their use for a scientific celebration garnished with well-cooked food, would no doubt have pleased the Count very much if he could have participated. For the academicians did well by themselves at the bicentenary, both in science and in wassailing, as the following account of the ceremonies will intimate.

Rumford also endowed in the Royal Society of London an award in recognition of investigations of light and heat, and the medalists have been selected from any country. Professor Robert Williams Wood of Johns Hopkins University is the holder of both the American and British medals. S. P. Langley was also so recognized, and the elder Lord Rayleigh twice won the British award. The Royal Society's medals prior to 1900 went to many scientists who bore famous and familiar names, such as Rumford (the first award, in 1802), Davy, Fresnel, Faraday, Arago, Stokes, Pasteur, Maxwell, Kirchhoff, Tyndall, Fizeau, Angstrom, Lockyer, Janssen, Cornu, Huggins, Abney, Langley, Hertz, Röntgen, and Oliver Lodge.

Since 1900 the recipients of the medal awards from the Royal Society have been the following:

1900. Antoine H. Becquerel. *Radiation from uranium.*
1902. Charles A. Parsons. *Steam turbine.*
1904. Ernest Rutherford. *Radioactivity.*
1906. Hugh L. Callendar. *Heat experiments.*
1908. Hendrik A. Lorentz. *Optics.*
1910. Heinrich Rubens. *Long wave radiation.*
1912. Kamerlingh Onnes. *Low temperatures.*
1914. Lord Rayleigh. *Thermodynamics.*
1916. William H. Bragg. *X-ray radiation.*
1918. Charles Fabry and Alfred Perot. *Optics.*
1920. Lord Rayleigh. *High vacua.*
1922. Pieter Zeeman. *Optics.*
1924. Charles V. Boys. *Gas calorimeter.*
1926. Sir Arthur Schuster. *Optics and terrestrial magnetism.*
1928. Friedrich Paschen. *Spectroscopy.*
1930. Peter Debye. *Specific heats.*
1932. Fritz Haber. *Thermodynamics.*
1934. Wander J. de Haas. *Low temperatures.*
1936. Ernest J. Coker. *Polarized light in engineering.*
1938. Robert W. Wood. *Physical optics.*
1940. Karl M. G. Siegbahn. *X-ray spectroscopy.*
1942. Gordon M. B. Dobson. *Upper air physics.*
1944. Harry R. Ricardo. *Internal combustion engines.*
1946. Sir Alfred Egerton. *Physical chemistry in technology.*
1948. Franz E. Simon. *Temperatures near absolute zero.*
1950. Sir Frank Whittle. *Jet propulsion of aircraft.*
1952. Fritz Zernike. *Phase contrast microscopy.*

The American Academy's list of medal recipients before 1900 includes the names of Alvan Clark (telescopes), Josiah Willard

Gibbs (thermodynamics), H. A. Rowland (wavelengths), S. P. Langley (solar radiation), A. A. Michelson (velocity of light), E. C. Pickering (stellar photometry), and Thomas A. Edison (electric lighting).* The full list since 1900 is as follows:

1900. Carl Barus. *Various researches in heat.*
1901. Elihu Thomson. *Electric welding and lighting.*
1902. George E. Hale. *Solar and stellar physics.*
1904. Ernest F. Nichols. *Radiation pressure.*
1907. Edward G. Acheson. *Electric furnace researches.*
1909. Robert W. Wood. *Optical properties of metallic vapors.*
1910. Charles G. Curtis. *Improvements in steam turbines.*
1911. James M. Crafts. *High temperature thermometry.*
1912. Frederic E. Ives. *Color photography.*
1913. Joel Stebbins. *Selenium cell photometry.*
1914. William D. Coolidge. *Invention of ductile tungsten.*
1915. Charles G. Abbot. *Researches on solar radiation.*
1917. Percy W. Bridgman. *Thermodynamics at high pressures.*
1918. Theodore Lyman. *Short wavelength radiation.*
1920. Irving Langmuir. *Thermionics and allied phenomena.*
1925. Henry N. Russell. *Researches in stellar radiation.*
1926. Arthur H. Compton. *Röntgen rays.*
1928. Edward L. Nichols. *Spectrophotometry.*
1930. John S. Plaskett. *Stellar spectroscopy.*
1931. Karl T. Compton. *Thermionics and spectroscopy.*
1933. Harlow Shapley. *Luminosity of stars and galaxies.*
1937. William W. Coblentz. *Heat and light technology.*
1939. George R. Harrison. *Improvements in spectroscopic techniques.*
1941. Vladimir K. Zworykin. *Invention of television devices.*
1943. C. E. Kenneth Mees. *Science of photography.*
1945. Edwin H. Land. *Polarized light and photography.*
1947. E. Newton Harvey. *Nature of bioluminescence.*
1949. Ira S. Bowen. *Interpretation of nebulium.*
1951. Herbert E. Ives. *Researches in optics.*
1953. Enrico Fermi. *Neutrons and mesons.*
1953. Willis E. Lamb, Jr. *Excited hydrogen atoms.*
1953. Lars Onsager. *Irreversible processes.*

The various Rumford committees can probably explain why the names of Curie, Planck, and Einstein do not appear in the Royal Society's list, or the last named in the list of the American Academy.

*A complete list of recipients of medal awards prior to 1950 by both the Royal Society and the American Academy, together with a listing of Rumford grants for research, is given in the Academy's booklet, " The Rumford Fund " (1950).

The Rumford Celebration began with the birthday banquet in the Great Hall of the Harvard Club of Boston. The program of the evening is reproduced on pages 260-263. The illustrated bill of fare was compiled by Professor Brown, under whose direction were also prepared the models of many of the inventions and contrivances of Count Rumford which were on display in the anteroom. Pictures of some of this equipment are reproduced in the bill of fare and more extensively in Professor Brown's biographical sketch.

The Rumford Medalists among the guests of honor, listed on page 263, most of whom were able to be present, were with one exception introduced by the present writer, who read for each the citation of the reasons for his selection, as engraved on the respective medals; the other guests were introduced by the President of the Academy, Edwin H. Land. The banquet, the Bicentennial Lecture, and the " pleasing flow of spirits " that graced the whole occasion, brought substantial satisfaction to the academicians and their guests.

The program of three scientific symposia and the evening session for the presentation of three Bicentennial Rumford Medals is reproduced on page 264. As is usual, the greatest value of the conference came from the informal out-of-meeting conversations of the participants. The conferees included not only those mentioned on the program but a considerable number of advanced students from the Boston community and specialists in heat and light from educational institutions and scientific industries. The Ford Foundation generously arranged to cover the travelling expenses of three special guests from abroad. To the executive officer of the Academy, Mr. Ralph Burhoe, and his staff, the committees are deeply indebted for effective management of all details of the Celebration.

Of the three symposia on Recent Developments, only the first on Thermodynamics actually dealt with scientific matters close to the interests of Count Rumford. Atomic spectroscopy and nuclear physics were for him non-existent as fields of research and providers of material for the subsequent rich technologies. The three fields are now so widely dispersed that the contributions collected here do not form a unit in any sense, except that they are all in the areas of the physical sciences and all contributions are concerned with

BICENTENNIAL

Birthday Banquet

IN HONOR OF

Benjamin Thompſon, *Count Rumford*

born in Woburn, Maſſachuſetts

March 26, 1753

American Academy of Arts and Sciences

at the Harvard Club of Boſton

March 26, 1953

BILL OF FARE

Defcribed and illuftrated by Benjamin Thompfon

"PEACE OF MIND, which is as effential to contentment and happinefs as it is to virtue, depends much upon order and regularity in the common affairs of life; and in no cafe are order and method more neceffary to happinefs (and confequently to virtue) than in that where the prefervation of health is connected with the fatiffying of hunger, an appetite whofe cravings are fometimes as inordinate as they are infatiable." (1796)

"ONE PORTION of foup:

of pearl barley	0 $\dfrac{1129}{1200}$ oz.	
of peas	0 $\dfrac{1050}{1200}$ oz.	
of potatoes	3 $\dfrac{84}{1200}$ oz.	
of bread	0 $\dfrac{1114}{1200}$ oz.	
of water	13 $\dfrac{127}{1200}$ oz."	

(1795)
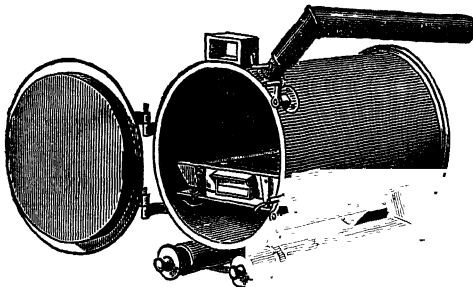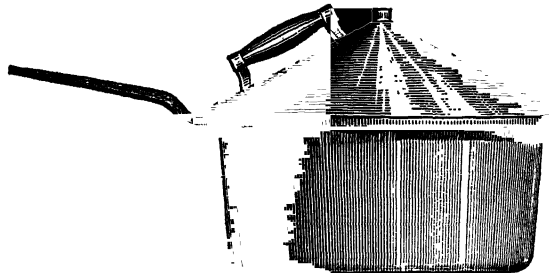
"HOWEVER BOLD the affertion may appear, I will venture to affirm that meat of every kind, without any exception, roafted in a roafter, is better tafted, higher flavoured, and much more juicy and delicate than when roafted on a fpit before an open fire." (1799)

" THERE IS NO ARTICLE uſed as food of which a greater variety of well-taſted and wholeſome diſhes may be prepared than of potatoes." (1796)
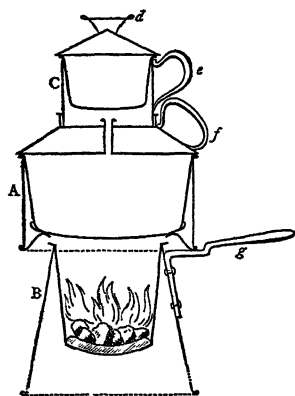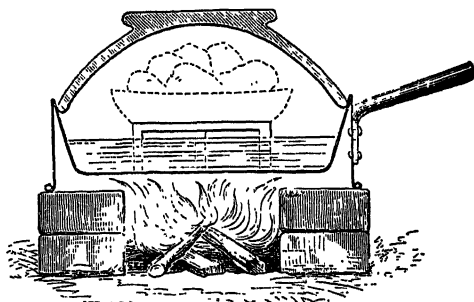
WHAT IN AMERICA is called a plain Indian pudding certainly holds the firſt place and can hardly fail to be much liked by thoſe who will be perſuaded to try it." (1796)

" AMONG THE NUMEROUS LUXURIES of the table unknown to our forefathers, which have been imported into Europe in modern times, COFFEE may be conſidered as one of the moſt valuable. Its taſte is very agreeable, and its flavour uncommonly ſo ; but its principle excellence depends on its ſalubrity and on its exhilarating quality. It excites cheerfulneſs without intoxication, and the pleaſing flow of ſpirits which it occaſions laſts many hours, and is never followed by ſadness, languor, or debility. It diffuſes over the whole frame a glow of health, and a ſenſe of eaſe and well-being which is exceedingly delightful. Exiſtence is felt to be a poſitive enjoyment, and the mental powers are awakened and rendered uncommonly active." (1812)
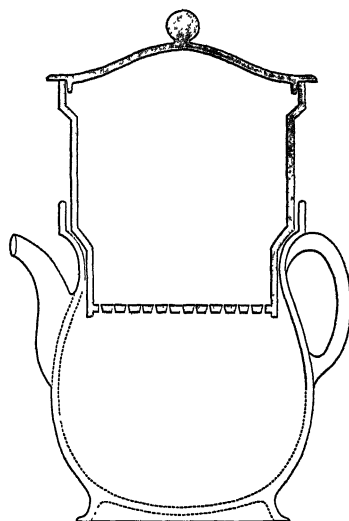
Guefts of the Academy

Rumford Medalifts of the
American Academy of Arts and Sciences

1909   Robert Williams Wood
1913   Joel Stebbins
1914   William David Coolidge
1915   Charles Greeley Abbot
1917   Percy Williams Bridgman
1918   Theodore Lyman
1920   Irving Langmuir
1925   Henry Norris Ruffell
1926   Arthur Holly Compton
1931   Karl Taylor Compton
1933   Harlow Shapley
1937   William Weber Coblentz
1939   George Ruffell Harrifon
1941   Vladimir Kofma Zworykin
1943   Charles Edward Kenneth Mees
1945   Edwin Herbert Land
1947   Edmund Newton Harvey
1949   Ira Sprague Bowen
1951   Herbert E. Ives
1953   Enrico Fermi
1953   Willis Eugene Lamb, Jr.
1953   Lars Onfager

Rumford Medalifts of the Royal Society

1930   Peter Debye
1940   Karl Manne Georg Siegbahn
1946   Alfred Egerton
1948   Franz Eugen Simon

Rumford Profeffor of Harvard Univerfity
Emory Leon Chaffee

Reprefentative of the Rumford Hiftorical Society
Richard C. Johnfon

Rumford Bicentennial Lecture
COUNT RUMFORD — *Phyficift and Technologift*
by Sanborn Conner Brown

RUMFORD BICENTENNIAL SYMPOSIA AND AWARDS
March 27 and 28, 1953

Symposium A
RECENT DEVELOPMENTS IN THERMODYNAMICS
Academy House
Friday Morning, March 27, 9.30 o'clock

Chairman: JOSEPH H. KEENAN, Massachusetts Institute of Technology
Vice-Chairman: MARK W. ZEMANSKY, City College of New York

P. W. BRIDGMAN, Harvard University
Reflections on Thermodynamics

SIR ALFRED EGERTON, Imperial College of Science and Technology, London
Management of Flame

LARS ONSAGER, Yale University
Reciprocal Relations in Irreversible Processes

FRANZ E. SIMON, The Clarendon Laboratory, Oxford
Helium and the Range of Stability of the Solid State

Awards
PRESENTATION OF THE RUMFORD BICENTENNIAL MEDALS
Academy House
Friday Evening, March 27, 8.15 o'clock

Presiding: EDWIN H. LAND, President, American Academy of Arts and Sciences
Master of Ceremonies: GEORGE R. HARRISON, Chairman, Rumford Committee, American Academy of Arts and Sciences

To ENRICO FERMI, University of Chicago
Introduced by PETER DEBYE, Cornell University
To WILLIS E. LAMB, JR., Stanford University
Introduced by NORMAN F. RAMSEY, Harvard University
To LARS ONSAGER, Yale University
Introduced by JOHN G. KIRKWOOD, Yale University

Reception for Rumford Medalists

Symposium B
RECENT DEVELOPMENTS IN ATOMIC SPECTROSCOPY
Academy House
Friday Afternoon, March 27, 2.30 o'clock

Chairman: DONALD F. HORNIG, Brown University
Vice-Chairman: ROBERT V. POUND, Harvard University

MARTIN DEUTSCH, Massachusetts Institute of Technology
Positronium

WILLIS E. LAMB, JR., Stanford University
Excited Hydrogen Atoms

EDWARD M. PURCELL, Harvard University
Line Spectra in Radio Astronomy

I. I. RABI, Columbia University
Molecular Beam Radiofrequency Methods for the Study of Excited States of Atoms

Symposium C
RECENT DEVELOPMENTS IN NUCLEAR PHYSICS
Academy House
Saturday Morning, March 28, 9.30 o'clock

Chairman: EDWIN B. WILSON, Office of Naval Research
Vice-Chairman: WALTER SELOVE, Harvard University

ENRICO FERMI, University of Chicago
Meson Physics

J. ROBERT OPPENHEIMER, Institute for Advanced Research, Princeton, N. J.
Recent Progress in the Understanding of Nuclear Forces

MANNE SIEGBAHN, Nobel Institutet för Fysik, Stockholm
Nuclear Spectroscopy

VICTOR F. WEISSKOPF, Massachusetts Institute of Technology
Problems of Nuclear Structure

The Rumford Committee
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new developments. Only the remarkable Reflections on Thermodynamics by Bridgman linked the historical past with the somewhat ragged thermodynamical concepts of the present. In fact, in several contributions, such as those by Egerton, Deutsch, Purcell, Oppenheimer, and Weisskopf, the science of the future came under constructive scrutiny more than did the preliminaries of the past.

Several of the reports printed in the following pages fall short of a full account of the contributions as presented in the symposia. A few of the communications came from exceedingly busy scientists who talked effectively but could not write papers or summaries for this number of the Proceedings of the Academy. Presumably their contributions will be fully presented later in the technical literature of their fields. The papers that are here included commemorate fittingly the New Englander of two centuries ago who honored our country, and other countries as well, by promoting the basic sciences that Heat our curiosity and Light our meditations on the nature of Nature's most important obscurities.

Rumford Bicentennial Lecture

Count Rumford —
Physicist and Technologist[*]

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Two hundred years ago, on March 26, 1753, a son was born to simple farmer folk in Woburn, Massachusetts. This child, Benjamin Thompson, seemed quite an average boy, and like all youngsters curious about scientific things, he played with electricity and fireworks as our present-day small boys play with electric toys and chemistry sets. His father, however, had died when he was only two, and by the time he was thirteen Ben was apprenticed as a clerk to an importer in Salem.

The importing business was not very prosperous in the 1760's. Mr. John Appleton's business was no exception, so that when young Benjamin was taken so ill that he was sent home for his mother's care, Mr. Appleton asked that he not return on his recovery.

Thompson's next job was with a dry-goods merchant in Boston who kept shop in the building that is now occupied by the Old Union Oyster House. Here he found that he was rather more interested in playing with gunpowder and inventing rockets and fireworks than he was in selling cloth and dry goods. When one of his gunpowder experiments blew up violently in his face, his employer, Mr. Samuel Capon, suggested forcefully that he look for employment elsewhere.

His scientific bent having by this time been recognized, he took up the study of medicine with a Dr. John Hay of Woburn. He seems to have really applied himself to the study of medicine, though one must confess that he seems to have spent more time in describing love potions in his notebooks with all the O's in the form of little hearts than in a serious effort to become a doctor. However it was while he was with Dr. Hay that he submitted his

[*] This paper was also published in the *American Scientist* of January, 1954.

first scientific paper for publication. This article, describing an abnormal child born in Woburn in 1771, was duly recorded as received by the American Philosophical Society of Philadelphia, but was never actually published. As a matter of fact, we know the details of this paper only by the minute description and sketch of this monster in Thompson's notebooks. Thompson was clearly intrigued by the more sensational aspects of his medical studies, but apparently found the work lacking in permanent appeal. In 1772 he left Woburn to teach school in Bradford, Massachusetts, where he found a guide and tutor in the local minister, the Reverend Samuel Williams.

One might say, parenthetically, a brief word about the checkered career of the Reverend Dr. Williams. An eminent fellow and charter member of the American Academy of Arts and Sciences, Dr. Williams became, some years later, Hollis Professor of Mathematics and Natural Philosophy at Harvard, a post which he held until he was accused of embezzling Harvard funds and was summarily dismissed by the overseers of that institution. To escape the public clamor, he fled to the Northern Wilderness, to found subsequently the University of Vermont.

But to return to our main subject, most of Benjamin Thompson's formal study seems to have been derived from contact with the Reverend Samuel Williams, and this was not restricted to the more conventional disciplines of mechanics, heat and light. He built Dr. Williams an electrical machine, and under his sponsorship sent another paper to the American Philosophical Society describing a " Remarkable Aurora Borealis " observed at Bradford in 1772. This aurora seemed more remarkable to Thompson than it did to the editors of the American Philosophical Society, who filed it with the comment: "Anyone living in our northern climes might observe fifty such displays in a year."

School teaching in Bradford failed to satisfy Thompson's need for something greater, or at least different, and in the summer of 1772 he departed to take a similar job in Concord, New Hampshire. Concord was populated mainly by families moving north from Woburn, and it was therefore not surprising that he came to Concord with the sponsorship of Concord's pastor, the Reverend Timothy Walker, himself an old Woburn native. It soon became evident that the patronage of the Reverend Mr. Walker was not

the only attraction in the Walker home, for Mr. Walker had a daughter, Sarah by name, who had just recently been widowed. The death of her husband, Colonel Benjamin Rolfe, left Sarah the richest landowner in Concord.

It took Thompson just four months to woo and wed the Widow Rolfe, fourteen years his senior. Since this marriage made him the proprietor of two-thirds of the land in the town, he found the job of schoolmaster somewhat superfluous. He wrote to his friends at the time that he was seriously taking up the science of husbandry, sending to England for great supplies of seeds and grains. We have records of Thompson's claims in court to lands whose titles had been clouded by the long legal dispute over the boundaries of the towns of Bow and Rumford. (Rumford's name had been changed to " Concord " to celebrate the fixing of the boundary lines of these formerly overlapping townships.)

Thompson's marriage to Sarah Rolfe had done more for his fortunes than just to make him wealthy. It hurtled him into the middle of the fashionable society surrounding the Royal Governor, John Wentworth, in Portsmouth. In this exciting environment Thompson found his true milieu. How he so successfully curried the favor of the Royal Governor will probably never be known, but curry it he certainly did. Within two years the lowly-born country schoolmaster from Bradford donned the handsome uniform of a major in the 15th Regiment of New Hampshire Militia. He was not quite twenty-one years old.

It must be confessed that the 15th Regiment of Militia was hardly more than a paper regiment, consisting as it did at its fullest of five commissioned officers, twelve non-commissioned officers, and no privates, but an intimate connection with any such organization was scarcely calculated to make Thompson popular with the revolutionary-minded patriots of Concord. To make matters worse, Thompson was strongly suspected of working actively for the British by hiring disguised British regulars to round up British deserters in the vicinity and ship them back to General Gage in Boston. It was hard to prove in 1774, but we know now that this was precisely what Thompson was doing.

The patriots of Concord did more than talk. After Thompson had been hauled before the local Committee of Safety, who could find no definite proof of disloyalty, the populace decided to take

matters into their own hands. Wisdom proved the better part of valor, and Thompson galloped abruptly away into the night on his brother-in-law's best horse to avoid the interesting experience of being tarred and feathered and ridden out of town on a rail by the irate citizens of Concord.

Back in his home town of Woburn, Thompson made no bones about his loyalty to the British Crown nor his feeling of injured pride at his treatment at the hands of the New Hampshire patriots. There still exists a remarkable record of his service to the British cause in the form of a secret-ink letter full of military information which he passed through the American lines to General Gage three weeks after the Battle of Lexington and Concord. If the letter had been intercepted, its author could easily have ended his career at the end of a rope, but because of the excellence of its technical chemistry, the message was as secure as any espionage activity can be. Like his erstwhile friends in Concord, the citizens of Woburn suspected that Thompson was more than an innocent bystander to the gathering revolution, but, as in Concord, the evidence was lacking. He was brought before the Massachusetts Committee of Correspondence, accused of what we now know he was doing, but no concrete proof could be produced, and he was released. He found it expedient, however, to leave Woburn shortly thereafter to join openly the British Loyalists in Boston.

Thompson's powers of observation served him well. In Boston his first act was to write a very detailed report on the " State of the Rebel Army," which outlined not only the state of morale, health, and ability of the American Army, but gave details of shipbuilding in Cambridge, the size and location of ammunition stores in the surrounding areas, their geographic vulnerability, and even how many guards were assigned to each. As a record of detailed and cogent reporting, it is a remarkable document.

How long Benjamin Thompson stayed in Boston before going to London we do not accurately know. We do know that he came very early to the attention of the Secretary for the Colonies, Lord George Germain, and rose very rapidly under his patronage. He was made Secretary of the Province of Georgia in December 1775 and had become Undersecretary of the Northern Department by September 1780. Concurrent with his meteoric rise in the British government he found more and more time for scientific

experimentation, primarily along military lines. He began at this time a long series of investigations on the force of fired gunpowder, and read his first paper on this subject before the Royal Society of London (of which he had become a member in 1779) in March 1781.

His interest in the force of fired gunpowder was naturally directed toward increasing the range of cannon balls fired from actual cannon. Since these measurements were difficult to do on land, Lord George Germain arranged for him to go on a cruise with Admiral Hardy's fleet to continue his studies under the more favorable conditions of naval bombardment. Not only did Thompson carry out experiments on the range of naval guns but he was profoundly shocked by the ridiculous inadequacy of marine signals. The event which really set him to thinking about the problem began with the sighting of a sail on the horizon which was thought to be part of the French fleet. The British admiral ran up what he considered to be an order for the ships to form immediately into the line of battle. To the consternation of those on the flagship, the response to the signal was a lowering of sails and the putting to sea of small cutters toward the Admiral's ship. What was supposed to be the order for line of battle turned out to be the signal for the weekly pay distribution!

Thompson worked out a system of signals that was apparently very good and was accepted by the British Admiralty, but disappeared because of security reasons, and we now know nothing about it.

All the evidence points to the fact that Thompson's interest was not solely connected with naval gunnery and signaling. A few months after his return to his office in London, a French spy by the name of La Motte was tried on charges of espionage on the British navy. At the trial the identity of a " person in a certain office " was not disclosed, but the gossip of the day pointed a finger at Benjamin Thompson, suggesting further that his powerful benefactor was shielding his identity. Whatever the precise facts in the case may be, we know that La Motte was publicly drawn and quartered at a well-attended and most impressive ceremony, and Thompson abruptly sailed for America as a lieutenant colonel in command of the yet-to-be-raised Loyalist regiment of the King's American Dragoons.

Colonel Thompson's executive ability was put to a very successful test in his collecting out of nowhere of a regiment of horsemen, raising it to full complement, and then retiring for the winter of 1783 upon the town of Huntington, Long Island. Unfortunately the populace of Huntington recalled nothing but evil of that winter. Thompson stabled his horses in the Presbyterian church overlooking the town. He pitched his own tent over the grave of the recently deceased minister, an ardent patriot, so that (according to his own testimony) he " could tread on the damn Yankee's head whenever he came in and out." He ordered his soldiers to make ovens of the gravestones so that they could distribute through the town loaves of bread upon whose lower crust the inverse inscriptions of deceased citizens were clearly legible.

At the close of the Revolution, Thompson returned to London just long enough to secure his government pension as a colonel on half pay before setting out as a soldier of fortune in Europe. After suitable negotiations he was invited to Bavaria as Inspector General of the Artillery and Aide-de-Camp to the Elector of Bavaria. He returned briefly to London to be knighted by George III so that his British rank would be commensurate with his Bavarian position, and finally settled down in Munich to his most profitable era of science and public administration.

Sir Benjamin was primarily a military man, and, as one would expect, many of his scientific investigations followed this direction. Because of his fundamental interest in heat and his belief that there existed some intimate relation between the force of gunpowder explosion and the heat produced, his studies in this field were extensive.

The standard method of testing the strength of gunpowder in his day was to fire cannon with various samples of powder under standard conditions, measuring the range of the shot. Since cannon were fired by powder trains and cannon balls fit loosely into the barrels of the guns, this method was far from a quantitative measurement. Thompson designed a device, shown in Fig. I, which became standard for many years thereafter.* Gunpowder was enclosed in a tightly closed bomb and fired into a closed system by a red-hot ball from the outside. The quantity of gunpowder was

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*All figures in this paper are taken from Count Rumford's published papers, except the photographs in Figures II and IX.
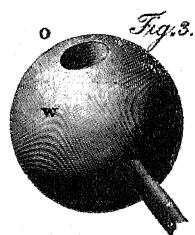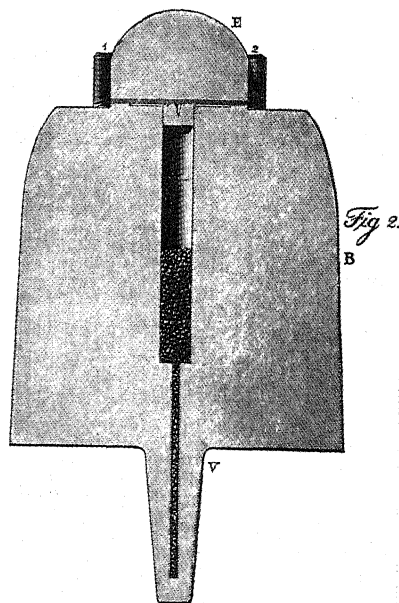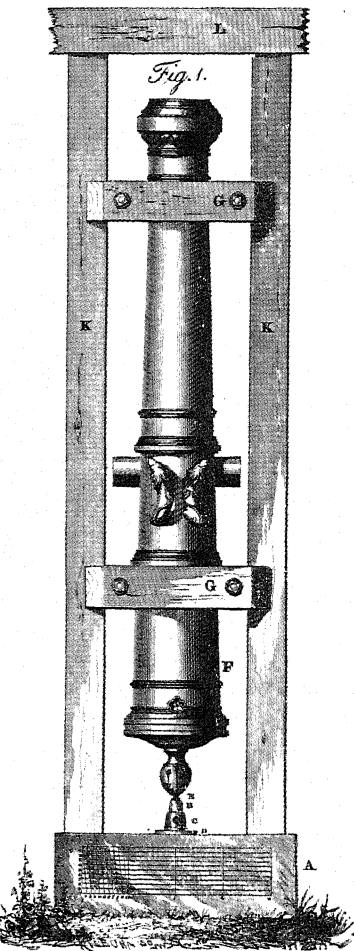
FIGURE I

adjusted so that the explosion would barely lift the bomb's tight-fitting cover, held in place by the weight of a four-ton cannon.

Having always been familiar with the heat evolved in firing guns and cannon, Thompson, even as a young man, was keenly interested in theories of heat. The generally accepted theory of the day was that heat was a subtle fluid called caloric. Thompson supported an opposing theory that heat was a form of energy, and devised experiments throughout his career to disprove the existence of caloric.

Thompson reported the most famous of his anti-caloric efforts in "An Inquiry Concerning the Source of Heat which is Excited by Friction." In this experiment he showed that the heat generated in the process of boring cannon was a definite, measurable quantity which did not diminish as long as the experiment was continued. He measured the heat produced by his cannon boring by measuring the time necessary for this heat to boil a given quantity of water. Fig. II shows the form his apparatus took. His conclusion was " that anything which any *insulated* body . . . can furnish *without limitation,* cannot possibly be a *material substance:* and it appears to me to be extremely difficult . . . to form any distinct idea of anything, capable of being excited and communicated in these experiments, except it be MOTION."

The expansion of a body on being heated was easy to explain on the material theory by the idea that the entering caloric took up space. Any demonstration which could show a body expanding on being cooled would be a clumsy problem for the caloricist. One such case is the behavior of water below 4°C, and Thompson carried out a long and careful series of experiments to show that water had its maximum density at 4°C, and would expand when cooled from that temperature downwards.

The caloric theory maintained that heat was a fluid which spread from hot objects to cooler ones and seemed to grow in extent because it was inherently self-repulsive. Caloricists felt that a lens could not produce an infinite temperature at its focus because the self-repulsive character of the caloric fluid should keep it from concentrating too much. To study the effect of converging and diverging rays, Thompson designed an instrument to measure the amount of heat in sun rays passing through lenses. His illustration of the receiver of his apparatus is shown in Fig. III. He pointed
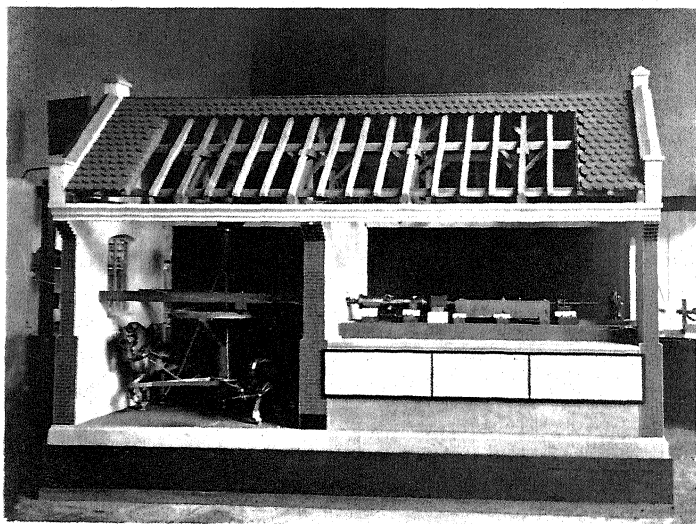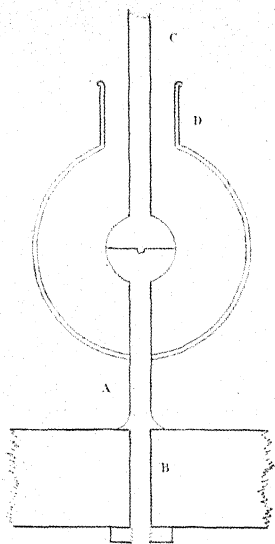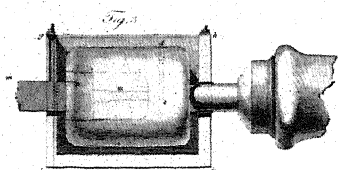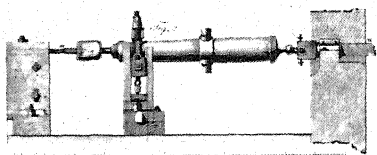
FIGURE II
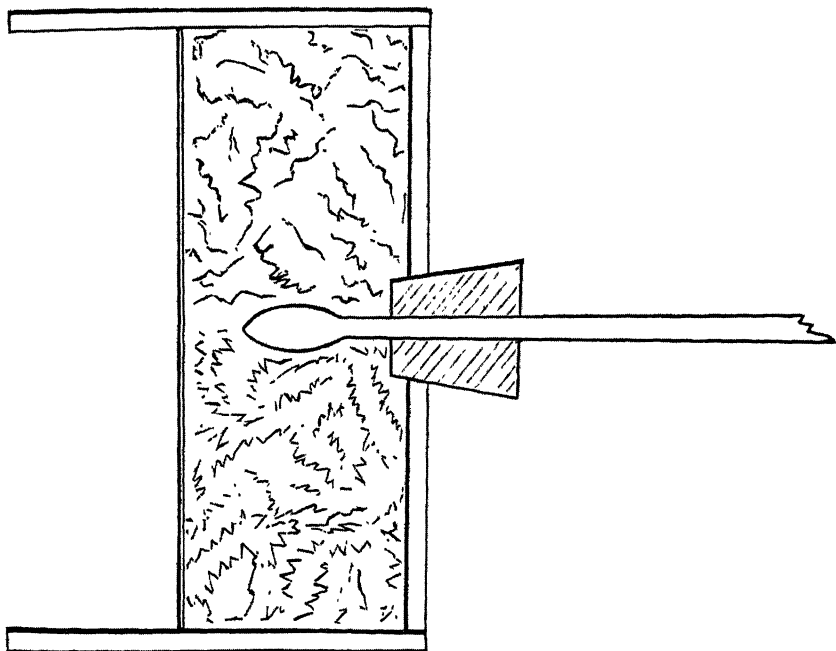
FIGURE III

his instrument toward the sun and measured the relative tempera-
ture rise of water contained in two metal boxes when lenses of the
same focal lengths were located at different distances from the
surface of the boxes. He showed that the amounts of heat passed
by two lenses of the same diameter is the same whether the rays
are parallel, diverging, or converging.

Caloric was considered to be conducted along a solid body by
being attracted by the particles of the body deficient in the caloric
fluid. Since this attraction seemed akin to a gravitational force,
the caloricists considered that heat should have weight. Sir Ben-
jamin disproved this theory by a most ingenious experiment. He
very carefully balanced two identical vials containing alcohol and
mercury respectively. He showed that they stayed in balance over
a temperature variation of over 50°F in spite of the fact that because
of the difference in the specific heat of the bodies, the quantity of

heat taken up by the mercury was very much greater than that absorbed by the alcohol.

Thompson carried out a long series of investigations on the " Propagation of Heat in Various Substances." He showed that his observations warranted the conclusion that in a single substance the conducting power increased with the compactness of that structure, as predicted by the caloric theory. Carrying the caloric theory of conduction to its logical conclusion, conduction through a vacuum should not occur since no attractive material particles are present. Thompson was the first to prove that this prediction of the caloric theory was false because he could demonstrate heat passing through a vacuum. Several models of apparatus designed to show this effect are illustrated in Fig. IV.

Although Thompson spent the greatest part of his scientific career trying to disprove the caloric theory, the energy theory of heat was not generally accepted for another sixty to eighty years. The reason for this appears to be that no general theory of conservation was evident in the embryonic theories of heat as a mode of motion. The conservation of caloric was a cornerstone of the material theory and therefore fitted into an over-all concept of nature. Lacking such a firm base, the energy theory of heat was not firmly established until the conservation of energy was made acceptable much later by Mayer and Helmholtz.

The failure of the natural philosophers of the day to accept his theoretical proofs of the energy theory of heat did not particularly disturb Sir Benjamin. He was fundamentally more interested in technological improvements than in pure theory, a point well illustrated by his statement: " I have taken particular care cautiously to avoid bewildering myself in abstruse speculations. I have often thought that I should be perfectly satisfied could I but obtain the exercise of the *authority* of a magician, even though I should not be permitted to know *how* the obedient spirits I should call up performed their business. I can conceive of no delight like that of detecting and calling forth into action the hidden powers of nature! — of binding the Elements in chains, and delivering them over the willing slaves of Man! Could I have had my choice, I should much rather have been the Inventor of a water-mill, than the Absolute Sovereign and Law-giver of the whole Human Race."

In social and political organization Thompson was a rigid ad-
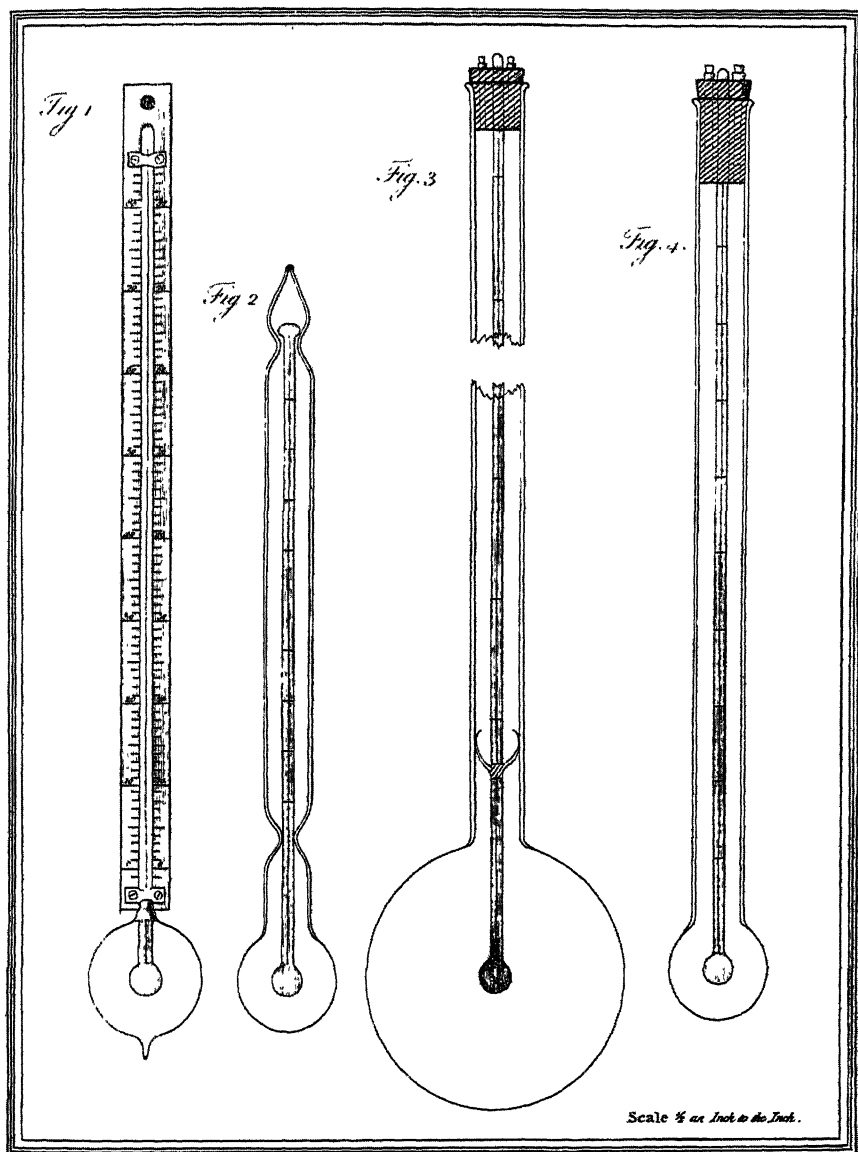
FIGURE IV

herent to order and efficiency, and throughout his regime in
Bavaria these principles dominated all his thinking. For reasons of
European politics, the Bavarian Army had to be numerically large,
but it was so poorly organized that it could not be used as a
fighting unit. The soldiers had very little to do, and hence morale
was sadly lacking. One of Thompson's first moves was to set up a
program in all its essential features like the late President Franklin
Roosevelt's W. P. A. He put the soldiers busily to work building
roads and public buildings as well as parks and gardens. He at-
tached to each military establishment a garden where the soldiers
were required to raise their own food, and he tried to teach them
the elements of scientific farming so that on their return to civilian
life improved methods of agriculture would be spread throughout
the country. The most elaborate of these projects, the English
Gardens in Munich, for which Thompson appropriated the Elector
of Bavaria's private deer sanctuary, is still one of the attractions of
the city of Munich.

His principle of order was profoundly shocked by the plague
of beggars which almost dominated the operation of the city of
Munich. With characteristic vigor and directness, Thompson ar-
rested all the beggars in the city, put them in a great prison-like
" House of Industry," and set them all to work making shoes and
clothes for the army. In his houses of industry where men, women,
and children slaved fourteen hours a day, he organized a public
school which all these children were required to attend. A hot-
lunch program was provided in this school. This state-administered
educational program is often considered the forerunner of our
present public-school system.

Some of his institutions were objects of great amazement to his
contemporaries. One of these is described by the famous early
American statesman, Gouverneur Morris, who visited Munich in
1798: " We go on to the house fitting up, under Count Rumford's
direction. This house was built for ladies to live in privately, and
is the most superb building in Munich. The idea is the most ex-
traordinary that I ever remember to have met with. It was further
intended for the education of those young scions of nobility which
had been furtively taken from the noble stock. In England this
would be called a strong legislative declaration of unchasteness."

To maintain the maximum of economy and efficiency for all his

military organizations and work houses, Thompson energetically applied his investigative genius to the sciences of nutrition, the economy of fuel and cooking, the science of illumination and clothing.

To feed the maximum number of people with the highest efficiency, Sir Benjamin experimented with many types of nutritious soups, and the so-called Rumford Soup which he finally developed is still served by that name in Europe. He introduced the potato into Bavaria and incorporated it into all of his recommended menus. He studied heats of combustion of the common cooking fuels and then designed stoves, roasters, and pots and pans to extract the maximum amount of heat from the fuel. His stoves were the antecedents of our present kitchen range, the evolution of which is illustrated pictorially in Fig. V. The first picture shows the open range in common use, which was installed in the opening of an ordinary fireplace. The next picture illustrates a Rumford register stove, in which the fire playing on the bottoms of the kettles could be regulated by a register. The next figure is his plan for a complete kitchen stove unit. Rumford Roasters were famous for many years and are still to be found occasionally in old New England houses. His drawings of this roasting oven are shown in the last two pictures. Fig. VI is a composite illustration of a number of Rumford designs for pots and pans to fit individual stoves. The first row shows the construction of various utensils. The second row shows what some of them looked like from the outside. Various stove constructions are given in the third row, and combinations are illustrated in the last row. The final picture of this row shows Rumford's design of what we know today as a " fireless cooker."

Thompson tells us that he spent fifteen years trying to make a truly superior cup of coffee. All this effort resulted in the " drip " coffee maker. Fig. VII shows various designs of his coffee pots. The first two pictures are his drawings of general purpose coffee makers. The second picture shows inserts of varying radius for different amounts of coffee, since he believed that the water should always pass through the same standard thickness of coffee. Coffee urns for large numbers of people are also illustrated. He designed the single cup device " for very poor persons who cannot afford to buy a coffee-pot."

Fig 48

Fig 19

Fig 1

Fig. 17

Fig. 19.

FIGURE V

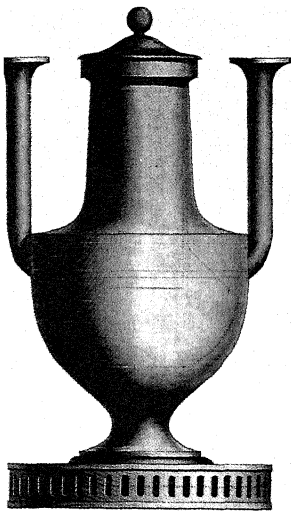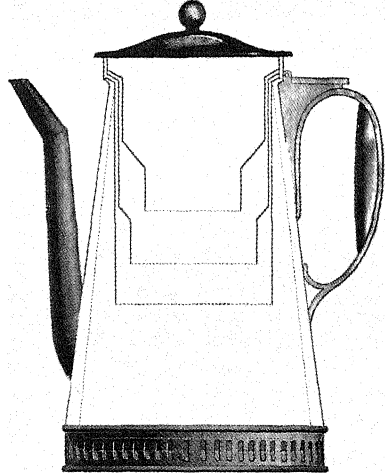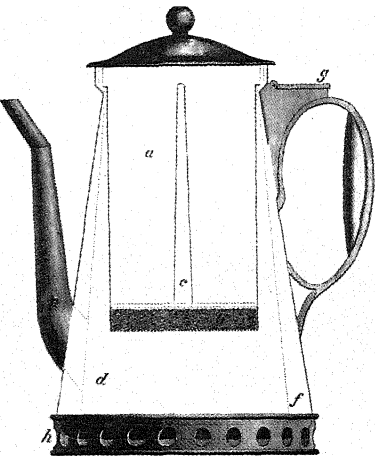Fig. 83. Fig. 84. Fig. 85. Fig. 53.
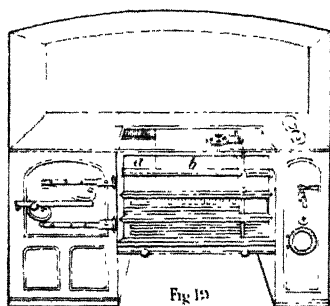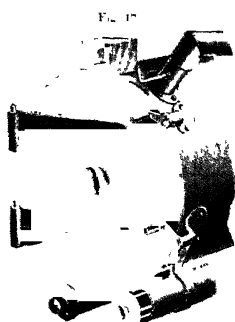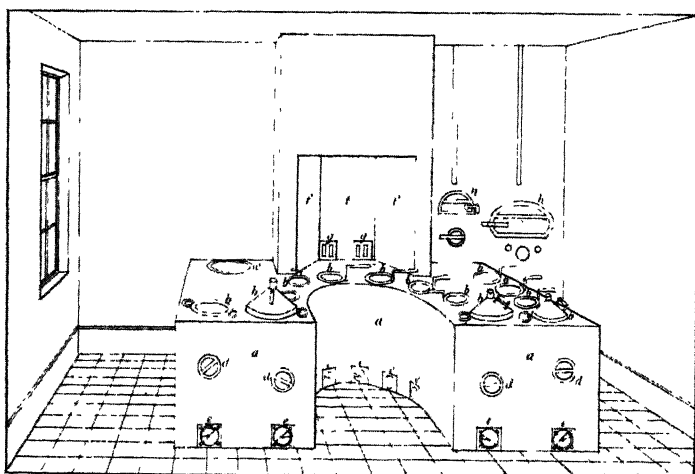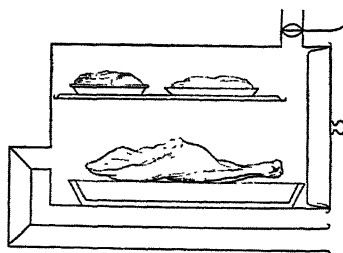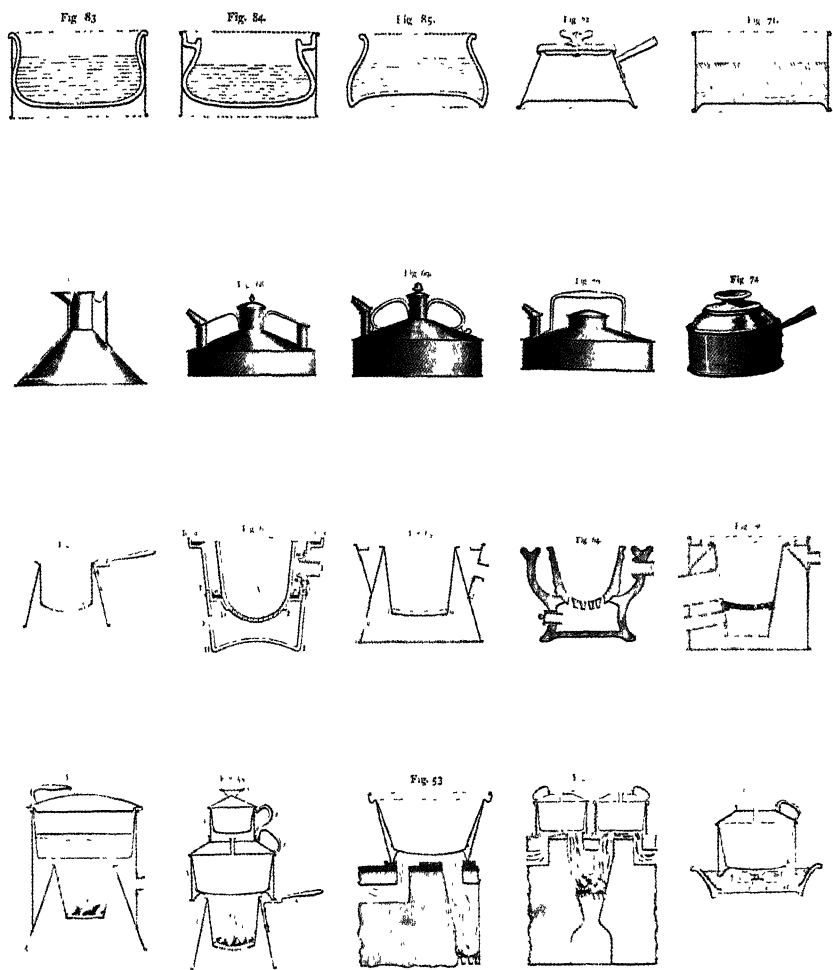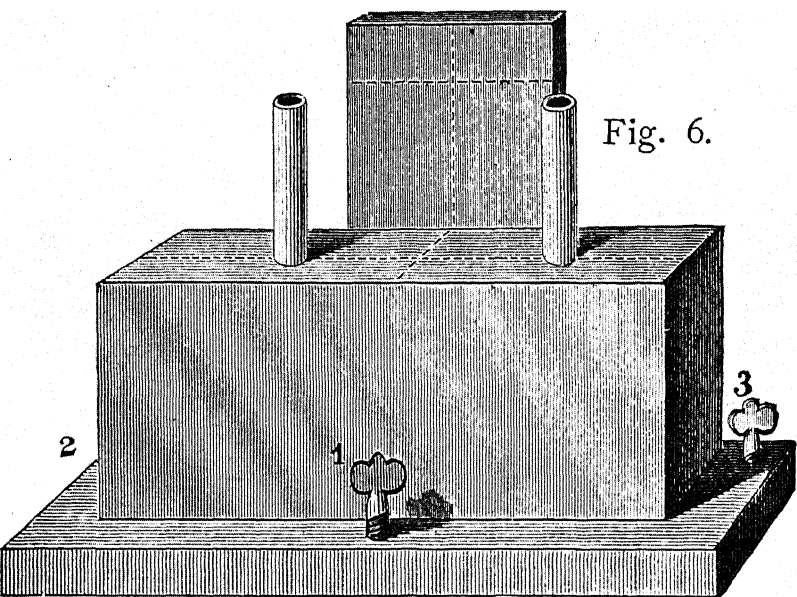
FIGURE VI

FIGURE VII

Fig. 6.
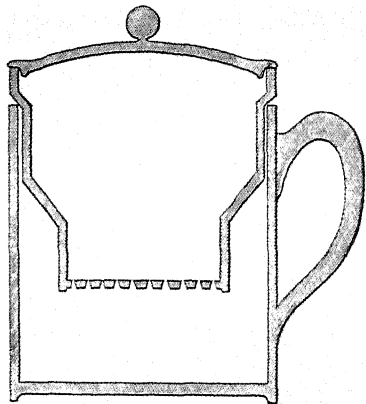
FIGURE VIII

To maintain working efficiency in his houses of industry, illumination needed to be greatly improved. First Thompson invented a shadow photometer illustrated in Fig. VIII. Two lights to be compared in intensity throw the shadows of the two little pillars on the vertical field of the photometer, and the distances of the lights adjusted to make the two shadows equally black. Since the intensity of illumination from the light source varies inversely with the square of the distance from the source, emitted intensities can be accurately compared. He defined a standard candle for use with his photometer which is almost identical to our present-day standard " candle."

Having designed instruments to measure light, Sir Benjamin turned his attention to improving lamps themselves. Lamps of that day had the oil reservoir directly below the burner, and the common whale oils were so viscous that as the lamp burned it became more and more difficult for the oil to rise in the wick. Thus

the light became steadily weaker. Rumford designed one lamp which maintained the oil height on the wick by feeding the burner from a circular reservoir in the form of a hollow flat ring which surrounded the burner and was level with it. He invented another which accomplished the same purpose by having the oil reservoir on a side arm at the height of the burner. Both of these types are illustrated in Fig. IX.

In order to clothe the soldiers of the Bavarian army properly and economically, Thompson studied the transmission of heat through and radiation from all kinds of material of which clothes and blankets were made. This led him into a study of practical insulation, experiments which received wide attention and had great influence on the thinking of the engineers of his time.

In recognition of all his great endeavors on behalf of Bavaria, the Elector raised Thompson in 1791 to a Count of the Holy Roman Empire, and for this Thompson chose the title of Rumford in honor of the former name of the New England town where his fortunes had changed so spectacularly. With fame came also fortune, and his title is perpetuated by two large gifts of money to the Royal Society of London and the American Academy of Arts and Sciences for endowing the Rumford medals and premiums.

Count Rumford's ruthless and tactless introduction of his improvements in the face of the established order in Bavaria won him bitter enemies on all sides, and by 1799 it was evident to the Elector that he would have to get rid of his able Count. To do this with honor, he appointed Rumford Bavarian Minister to the English Court and Rumford left for London. However, this appointment was so unacceptable to George III (ostensibly because Rumford was a British subject, but really because of his previous behavior) that the Count could not even get an appointment to talk the matter over with the King's ministers. The Count thus found himself in London without a job, but he quickly busied himself with several projects. He applied through his friend Rufus King, American Ambassador to the Court of St. James, for the position of Superintendent of the U. S. Military Academy. This was looked on with great favor until investigation showed his past intelligence activities. Matters were then carefully arranged so that he would be offered the position publicly on the private assurance that he would refuse, which he did.
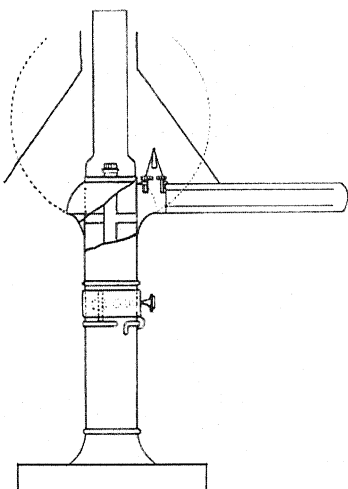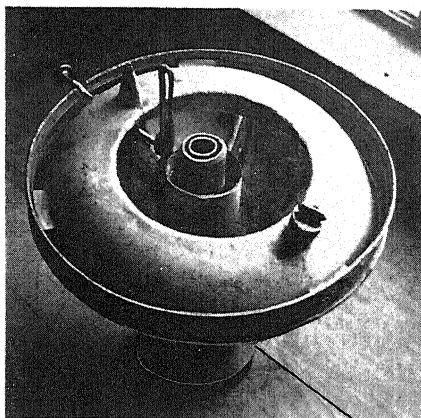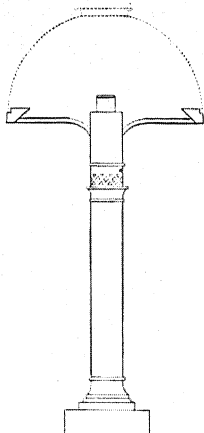
FIGURE IX

As many before him had done, he turned his attention in England to the perennial problem of smoky chimneys. Chimneys of his day were little more than large holes or flues connecting a fireplace to the roof above. In a series of brilliant deductions, Rumford reasoned that chimneys smoked because there were no clearly defined paths for the rising and descending air currents. The smoke shelf and throat which he introduced separated the warm from the cool air and set up orderly convection currents (currents which he himself had discovered some years before). "Before and after" drawings of his improvements are shown in Fig. X. So completely did Rumford solve the problem that essentially no improvements have been made in chimney construction since.

The occupation into which Count Rumford threw himself wholeheartedly around 1800 was the founding of the still famous Royal Institution of Great Britain. This was a scientific institution which, much like our Museums of Science today, was established to bring before the public the scientific and technological advances of the times. The Prospectus of the Royal Institution outlines its function and operation in the greatest detail. In listing those specific subjects which should be followed with the utmost vigor, one finds a complete listing of those facts of science and technology which interested Rumford to the total exclusion of those that did not. In building the lecture hall for the new institution, the Count designed the first successful steam-heating system and installed it as a working model for all to see.

Count Rumford's insistence that every detail of the operation of the Royal Institution be carried out exactly as he desired it brought about an intolerable situation within a very few years. The struggle between Rumford and others who were also responsible for the growth and operation of the institution resulted in a showdown for which Rumford never forgave his associates. He resigned in great anger, vowing that he would leave England never to return, a threat which he carried out.

He left London for Paris, where on previous visits he had been royally treated by French society and had found particular favor in the company of the widow of the famous French chemist Lavoisier. Not long after he settled in Paris he and Mme. Lavoisier married and his position in Parisian society appeared assured. However, Rumford's personality had never been an easy one to get
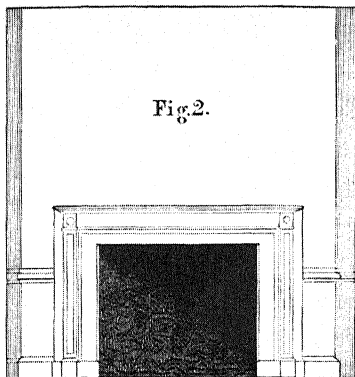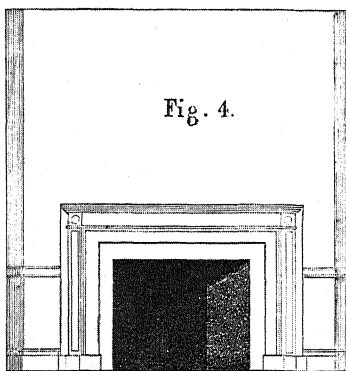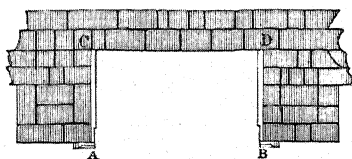
Fig. 2.

Fig. 4.

Fig. 1.
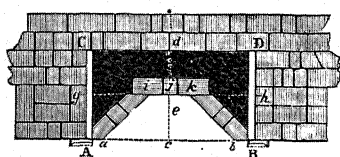
Fig. 3.

Scale of ||||||||| 1 2 3 4 5 Feet.

Scale of ||||||||| 1 2 3 4 5 Feet.
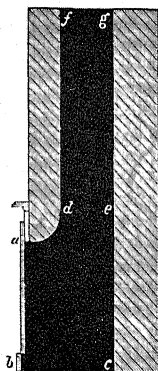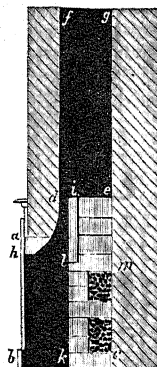
Fig. 5.

Fig. 6.

FIGURE X

along with, and age seemed to accentuate his eccentricities. He became embroiled in lawsuits over his improvements in lamp design, used his seat in the French Academy to oppose bitterly the scientific findings of numbers of French physicists and chemists, and fought publicly with his wife, who refused to abandon the name of Lavoisier in favor of Rumford. The story was told of how on one occasion a large party of guests rolled up to the gate of the estate, bidden by Madame to a soiree. Rumford saw them approach, rushed out, and ordered the gatekeeper to slam the gates in their faces. Then with a flourish he produced a key, locked the gate, and hurled the key into the river. Mme. Lavoisier retaliated by ordering the cook to prepare great vats of boiling salt water, which she poured over Rumford's prize rose gardens. After the formal separation took place, Rumford retired from the public view to the Parisian suburb of Auteuil, where he lived almost as a recluse, turning his whole attention to scientific pursuits.

In his last years at Auteuil, Rumford produced a tremendous number of papers on a very wide range of subjects. The following is only a partial list of titles of the period: " New Experiments in the Propagation of Heat in Fluids," " Use of Steam Heat in Distilling Brandy," "Adhesion of Water Molecules," " New Boiler to Save Fuel," " Steam Heat in Making Soap," " Spontaneous Mixture of Liquid " (a paper in which he first demonstrated the interdiffusion of two liquids of different density), " Cooling of Liquids in Gilded and Ungilded Vessels," "Advantages of Wheels with Wide Felloes," " New Lamp," " Experiments with Wood and Charcoal," " Light from Combustion of Inflammable Bodies " (in which he demonstrated that the light from a candle came from incandescent solids), " Heat Developed in Combustion," " Heat of Condensation of Alcohol," " Heat Capacity of Liquids," and " The Structure of Wood."

The most fitting way to close this sketch of Count Rumford is to quote from a fellow scientist who knew the Count most intimately, and who had his confidence for many years. In pronouncing Rumford's eulogy before the French Academy, Baron Cuvier said of him:

" Nothing would have been wanting to his happiness, had the amenity of his behavior equalled his ardor for public utility. But it must be acknowledged that he manifested in his conversation

and in his whole conduct, a feeling which must appear very extraordinary in a man so uniformly well treated by others, and who had himself done so much good. It was without loving or esteeming his fellow creatures that he had done them all these services. Apparently the vile passions which he had observed in the wretches committed to his care, or those other passions which his good fortune had excited among his rivals, had soured him against human nature. Nor did he think that the care of their own welfare ought to be confided to men in common. That desire which seems to them so natural, of examining how they are ruled, was in his eyes but a factitious product of false knowledge. He considered the Chinese government as the nearest to perfection, because, in delivering up the people to the absolute power of men of knowledge alone, and in raising each of these in the hierarchy, according to the degree of his knowledge, it made in some measure so many millions of hands the passive organs of the will of a few good heads.

"An empire, such as he conceived, would not have been more difficult for him to manage, than his barracks and poor-houses. For this he trusted especially to the power of order. He called order the necessary auxiliary of genius, the only possible instrument of real good, and almost a subordinate divinity regulating this lower world. He himself, in his person was, in all imaginable points, a model of order. His wants, his labors, and his pleasures were calculated like his experiments. In short, he permitted in himself nothing superfluous, not even a step or a word, and it was in the strictest sense that he took the word *superfluous*.

" This was no doubt a sure means of devoting his whole strength to useful pursuits, but it could not make him an agreeable being in the society of his fellows. The world requires a little more freedom, and is so constituted that a certain height of perfection often appears to it a defect, when the person does not take as much pains to conceal his knowledge as he has taken to acquire it.

" This rigorous observance of order, which probably marred the pleasures of his life, did not contribute to prolong it. A sudden and violent fever carried him off at the age of 61. He died on the 12th August 1814 in his country house at Auteuil."

The Scientific Work of Enrico Fermi

PETER DEBYE
Cornell University

This statement concerning the work of Dr. Fermi was made on the occasion of his being awarded the Rumford gold and silver medals for his studies of radiation theory and nuclear energy by the American Academy of Arts and Sciences in Boston on March 27, 1953.

Fifty-one and a half years ago, on the 29th of September, 1901, Enrico Fermi was born in Rome. He studied at the University of Pisa. After completing his studies in 1922, he went to Göttingen, worked with M. Born, and traveled as far north as Leiden. In 1924 he was appointed a lecturer at the University of Florence.

As we look at his work during these first years of his career we meet with publications handling problems often discussed at that time. Some physicists were troubled by the fact that apparently a nasty infinity crops up when the thermal equilibrium of a hydrogen nucleus and its electron is considered. Fermi shows that there is no trouble when the actual case of many hydrogen atoms at finite distances from each other is considered.

Another problem, treated previously by N. Bohr, concerned electrons or a-particles traveling through a gas. Fermi handled the problem in developing the electrical field of the moving charge at the position of the gas atom or molecule in a Fourier time-series, and therefore, in essence, handled the case as if the atom or molecule were exposed to light waves of all frequencies, from zero to infinity. Characteristic for the state of quantum-theory at that time is the way in which Fermi helps himself: he cuts off the series at a frequency determined, by means of Planck's quantum, by the kinetic energy of the flying particle.

With Rasetti he considered and treated the effect of a magnetic field on resonance-radiation in the interesting case in which the field is not constant but periodic in time.

In 1926 Fermi published a paper which was of great importance for the further development — a real hit. It is entitled (in trans-

lation), "Concerning the Quantization of the Ideal Monatomic Gas." In 1925, inspired by a paper published by Stoner a year before, Pauli had formulated the "Pauli principle," according to which one quantum state cannot accommodate more than one electron. Fermi observed that the classical theory of the Equation of State of ideal gases does not agree with the Nernst theorem, which requires the entropy of all substances in thermal equilibrium to be essentially equal, say zero, at the absolute zero of temperature. He saw that if the Pauli principle is applied to gas atoms, this discrepancy can be avoided and fundamentally important deviations of the classical laws of ideal gas behavior have to appear at sufficiently low temperatures. The reasoning employed by Fermi is of course based on Boltzmann's relation between entropy and probability. However, the way in which the counting is performed in order to find this so-called probability is and has to be totally different from the counting which Boltzmann used.

I could not but feel, when for the purpose of this introduction I read the paper again, that Fermi, when he wrote it, must have been disturbed a good deal about the fact that for ideal gases his calculation gives deviations from the classical laws of opposite sign to those calculated a short time before by Einstein for the same case. This discrepancy only shows how much we still had to learn in 1926. We now know that both Fermi and Einstein were right; we have Bose-Einstein and Fermi-Dirac statistics, and which one to use depends on the parity of the objects. For electrons, Fermi's theory applies and it is hard to imagine what we could do nowadays about the understanding of, say, conductivity in metals and semi-conductors without knowing about the Fermi-level.

In 1927 Fermi became Professor of Theoretical Physics at the University in Rome. He married in 1928 and stayed in Rome until shortly after receiving the Nobel Award, when he transferred to Columbia University in 1939 and to the Institute of Nuclear Physics in Chicago in 1945.

During his first years in Rome Fermi was still mostly interested in atomic and molecular structure — in the outside of the nucleus. He wrote about the analogy in quantum theory of the impact of a mass-point on a rotator, and on the optical theory of a grating. He showed how from the observation of the anomalous dispersion in the vicinity of two thallium lines the quotient of Planck's constant

h to Boltzmann's constant *k* can be determined. He demonstrated how dependent the calculated intensity quotient of the alkali doublet is on the choice of the eigenfunctions used in the calculation, and explained in this way the observed quotients different from the ratio 2:1. He showed how an observed double, instead of an expected single line in the Raman spectrum of the linear molecule of CO_2, can be traced back to an accident concerning the numerical value of the quotient between the frequencies of two possible modes of vibration of the molecule. With Rasetti he demonstrated by a very ingenious experimental method the Raman effect of rock salt, which, according to a first-approximation theory, should not exist at all.

However, in 1933 the atomic nucleus began to appear in Fermi's thinking. First he is still outside when with Segré he considers the effect of magnetic nuclear moments on the hyperfine structure of spectral lines. One year later he is already deep inside when he publishes his theory of β-decay. Immediately following comes his famous series of publications that tell (1) of artificial radioactivity induced by neutron bombardment; (2) of how neutrons can be slowed down by giving them an opportunity to collide with H nuclei in passing them through materials composed of molecules that contain much hydrogen; (3) of the possible production of elements with atomic numbers exceeding 92.

The ensuing story of the enormous development which occurred during Fermi's time at Columbia University and which led to neutron chain-reactions and the construction of the so-called " atomic " bomb is too well known and too fresh in our minds to be dwelt upon at this time; it can only be reverently mentioned.

This evening Fermi receives a medal founded by a man with a great scientific mind, who at the same time was an international character. It seems symbolic that you are witnessing here in the heart of Boston the spectacle of one Holland-born introducing one Italian-born to receive a reward brought into existence by the munificent gesture of one American-born, who was a Bavarian Count. It is presented to the recipient for his accomplishments in the field of light and heat. Fermi made possible the highest concentration of light and heat in one point ever man-made. In this Academy we value no less his scientific deeds. Thinking about physicists who are trying to distill quantitative theories and pre-

dictions from the observed facts, it soon becomes apparent that we have to make a distinction between mathematical and theoretical physicists. The first rides the wild horses of mathematics as at a rodeo and believes in an active predestination that will take care to see him deposited in the right spot. Occasionally it even works out all right. The other — he rides the horses too, but he knows how to tame them and force them to go his way. This is how I see Fermi doing it with consummate skill.

The Scientific Work of Willis E. Lamb

NORMAN F. RAMSEY
Harvard University

This statement concerning the work of Dr. Lamb was made on the occasion of his being awarded the Rumford gold and silver medals for his studies of the atomic hydrogen spectrum by the American Academy of Arts and Sciences in Boston on March 27, 1953.

Professor Lamb received his doctoral degree from the University of California in Berkeley in 1938 for his theoretical research under the supervision of J. Robert Oppenheimer. From Berkeley, he went to Columbia University where he remained for the next twelve years. From Columbia, Lamb went to Stanford University where he now resides. In the few years that elapsed between his Ph.D. degree and the beginning of the war, he wrote various theoretical papers and did a superb job of teaching at Columbia.

Of Lamb's papers written during the pre-war period, I should like chiefly to speak of two. These have the rather intriguing characteristic that at the time they were written they appeared to be relatively unimportant and of limited application to a single small field. Only in the recent post-war years have physicists come to realize that each of these papers was fundamental in what has grown to be a large and important field.

One of these pre-war papers was Lamb's discussion of the magnetic shielding of nuclei in atoms. When the magnetic moment of a nucleus is measured by its interaction with an external magnetic field, an important correction must be made for the fact that the diamagnetic circulation of the atomic electrons also produces a magnetic field at the nucleus. Therefore, the nucleus precesses not alone in the known external magnetic field but in the resultant of this field and the unknown local field due to this induced electron circulation. Professor Lamb's paper was the first one which provided a means for calculating the correction for the induced magnetic field. At the time of this paper it appeared that Lamb's theory was of rather limited application to a few molecular beam experi-

ments. However, in recent years new techniques of nuclear moment measurements have greatly expanded the field. In addition, Lamb's fundamental paper on magnetic shielding has been extended to polyatomic molecules. The application of these theories to measurements with such molecules have in the past few years developed into a very fruitful means for studying molecular structure.

A similar history has accompanied the early paper of Lamb and of B. T. Feld on nuclear quadrupole moment interactions in molecules. Originally this paper was thought to have applications to only a few molecules which enter into molecular beam experiments. Subsequently, however, the paper of Lamb and Feld has been the foundation for many theoretical papers interpreting both nuclear resonance experiments and the extensive experiments of microwave spectroscopy.

During the last war, Professor Lamb developed both theories and magnetrons in the Columbia Radiation Laboratory. Indeed this work was his real start in experimental physics. There was only one rule in this laboratory and that was that everyone had to make magnetrons; so Lamb — though a theorist — made his, and they were good ones, too. Those of you who heard Lamb's talk today * could see from his slides how he profited from his Radiation Laboratory experience, since such apparatus could have been designed only by a man experienced in the techniques of sealed-off vacuum tubes.

After the war, Lamb began at Columbia his most important experiment and, indeed, one of the most significant of all post-war experiments. He measured with high precision the energy separation of the different levels of atomic hydrogen in its second excited state. Although the separations of these levels had been studied optically by Houston and others, the difficulty of the optical experiments was so great that contradictory results were obtained by different observers. Lamb, however, measured directly the separations by allowing microwave radiations to be emitted and absorbed. In this way he could make his measurements with high precision and found a marked disagreement with the Dirac electron theory. In particular, for two levels whose

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*Summarized in a later chapter.

frequency separation should have been exactly 0 megacycles
by Dirac's theory, Lamb found the far different result of
1057.77±0.10 megacycles, which is frequently spoken of as the
Lamb shift.

Once this value was found experimentally, theorists were able by
a process of "postdiction" (as opposed to prediction) to re-
examine their theories and say that they should have suspected
such a result all along. However, this very process of re-examina-
tion has led to one of the major theoretical developments of recent
years — namely, the development of suitable procedures for mak-
ing quantum electrodynamic calculations. According to these
"postdictions," the source of the Lamb shift was the interaction of
the electron with both the electromagnetic field and the field of
negative energy electrons which affect an electron even when the
fields are in their lowest state. The precise calculation of the Lamb
shift is a very complicated one indeed and occupied theoretical
physicists for several years. It is significant to note that the first
"postdicter" to reach and publish the correct answer was also
Willis Lamb, functioning this time as a theorist instead of as an
experimentalist.

Another important measurement to come from the same experi-
ment was the precision value for the fine structure constant,
$a = e^2/\hbar c$, one of the intriguing dimensionless numbers which
so frequently occurs in quantum electrodynamics. His result was
$1/a = 1/(137.0365 \pm 0.0012)$.

The only debatable point about the award's going to Professor
Lamb was the question of whether or not his research was really
in the field of light and heat. For three different reasons the com-
mittee felt it was. One reason is that microwaves are merely an
extension of the same electromagnetic radiation as ordinary light
and are merely in a frequency interval unknown in Count Rum-
ford's day. A second reason is that the same energy levels studied
by Lamb can be studied by means of optical spectroscopy. A
measurement of these same quantities with a hundredth of Lamb's
accuracy by optical means would have been a perfect basis for the
Rumford award, and it seemed inappropriate to withhold the
award from Lamb merely because of his much greater accuracy.
Finally, it should be noted that in 1926 the premium was given to
Professor Arthur Compton for his X-ray studies. His wavelengths

are shorter than the optical ones by as much as Lamb's are longer. In fact, it is of interest to observe that the geometric mean between the wavelengths characteristic of X rays and those of microwaves is just within the visible spectrum. So even if Lamb's experiments were not regarded as strictly within the fields of light and heat, the award would be justified as a righting of the past wrong of Professor Compton's award.

The Scientific Work of Lars Onsager

JOHN G. KIRKWOOD
Yale University

This statement concerning the work of Dr. Onsager was made on the occasion of his being awarded the Rumford gold and silver medals for his contributions to the thermodynamics of transport processes by the American Academy of Arts and Sciences in Boston on March 27, 1953.

Lars Onsager began the study of science at the Norwegian Technical Institute in Trondheim, Norway, from which he received the degree of Chemical Engineer in 1925. He then went to Zürich, where he worked under the direction of Peter Debye during the period 1926-1928. He came to the United States in 1928 as Associate in Chemistry at the Johns Hopkins University. Shortly thereafter he went to Brown University as Research Instructor, where he remained until 1933. In 1934 he became Assistant Professor of Chemistry in Yale University, and since 1945 has held the Willard Gibbs Professorship of Theoretical Chemistry at Yale. He received his Ph.D. from Yale University in 1935.

The early papers of the Debye-Hückel theory of electrolyte solutions appeared during Onsager's period of study at Trondheim. They caught his attention and led to his early interest in the theory of electrolyte solutions, to which he has since made so many important contributions. Working independently in Norway, Onsager achieved certain important and necessary improvements in the theory of electrolytic conductivity, which had already received preliminary treatment by Debye. He completed this work in Zürich and published the results in 1926 and 1927. His interest in electrolytes continued, and in 1932 he published with Fuoss a definitive paper on irreversible processes in electrolyte solutions, in which a complete and general theory of electrolytic conductivity and diffusion in multi-valent electrolyte mixtures was presented. In 1933, Onsager published a general critique of the Debye-Hückel theory, based on the principles of statistical mechanics. By investigating the conditions of integrability of the average

electrostatic potential in the neighborhood of an ion, he was able to meet certain objections which had been raised against the theory by R. H. Fowler and to show that the linearized form of the Debye-Hückel theory was consistent with statistical mechanics. In this same field Onsager achieved a theoretical treatment of the Wien effect in weak electrolytes and, in collaboration with Samaras, a rigorous treatment of the surface tension of electrolyte solutions.

While still a student in Norway, Onsager began to give thought to the general theory of irreversible processes. He continued to develop his ideas on this subject during his stay in Zürich and during his early years in the United States. In 1931 he published two important papers on irreversible processes, in which he formulated his reciprocal relations, which form the basis of irreversible thermodynamics. For small departures from thermodynamic equilibrium, the currents of heat and matter may be expressed as linear functions of the gradients of temperature and of the chemical potentials of the several components of a system. Onsager formulated the general principle, earlier foreshadowed by Thompson in his treatment of thermoelectric potentials, that the matrix of the coefficients relating the currents to the gradients of the potentials is symmetric. This principle may be regarded as a phenomenological law, with the same status as the first and second laws of thermodynamics, to be verified directly by experiment. As Onsager demonstrated, it may also be regarded as a consequence of a more general principle, the principle of least dissipation, which states that the rate of increase of entropy arising from a set of coupled irreversible processes is a minimum. The principle may be justified from the molecular point of view on the basis of considerations arising from the principle of microscopic reversibility.

In 1936 Onsager published an important paper on the dielectric polarization of polar liquids, in which he demonstrated that a drastic modification in the Lorentz internal field was necessary in polar dielectrics. His simple and penetrating analysis of this problem swept away puzzling paradoxes, arising from the incorrect use of the Lorentz field, and clarified the relationship between the dielectric constants of polar liquids and their structure on the molecular scale. Among the important consequences of this theory was the correct interpretation of the dielectric constant increments pro-

duced in aqueous solution by the amino acids and proteins, which possess large permanent electric dipole moments.

In 1940, Kramers and Wannier demonstrated that the partition function of a crystal consisting of N atoms could be expressed as the Nth power of the largest eigenvalue of a matrix operating on a vector in a space having a number of dimensions equal to the number of states accessible to each atom. They applied their method to the calculation of the partition function of a linear chain of atoms in the problem of classical ferromagnetism. However, extension of their method to two- and three-dimensional arrays of atoms involves extraordinary and formidable mathematical difficulties. In a paper in 1944 on crystal statistics, Onsager, accomplishing a feat of remarkable mathematical virtuosity, was able to express the partition function for a two-dimensional array of classical ferromagnets in the closed form of a definite integral. In a later paper in 1947, written in collaboration with Miss Kaufmann, the mathematical treatment of the problem was materially simplified with the aid of the theory of spinors.

In addition to Onsager's contributions to statistical mechanics and thermodynamics, which we have chosen to discuss here, he has made a number of other important contributions, among them in 1948 a theory of long-range orientational order in solutions of tobacco mosaic virus, in 1939 a contribution to the theory of thermal diffusion, and in 1952 an explanation of the Haas-Van Alphen effect. He is currently engaged in work on problems relating to Liquid Helium II, the properties of which are a macroscopic manifestation of quantum mechanics.

Onsager's scientific work is characterized not only by mathematical virtuosity but also by profound physical insight and intuition. His accomplishments are impressive and of lasting value. It is therefore most appropriate that he has been selected for a Rumford Premium Award by the American Academy of Arts and Sciences on the occasion of the 200th anniversary of Count Rumford's birth.

Reflections on Thermodynamics[*]

P. W. BRIDGMAN
Harvard University

The two laws of thermodynamics are, I suppose, accepted by physicists as perhaps the most secure generalization from experience that we have. The physicist does not hesitate to apply the two laws to any concrete physical situation in the confidence that nature will not let him down. But although he applies the laws with sureness, it does not necessarily follow that he could justify with equal confidence the soundness of his formulation of the laws or reproduce the argument on which the laws rest. The average physicist does not feel the need to give much attention to this sort of thing, and it would therefore not be surprising if the logical foundation on which thermodynamics rests is less satisfactory than the superstructure. This sort of situation is not unique for thermodynamics or for physicists. Most mathematicians, for example, in their moments of candor, will admit that they are much less confident of the logical foundations of mathematics than they are of its body of practise. The historical development of thermodynamics has been such, however, that it has been particularly susceptible to logical insecurity, since the laws were formulated in their present form by the great founders of thermodynamics, Kelvin and Clausius, before all the essential physical facts were in, and there has been no adequate reexamination of the fundamentals since.

The purpose of this talk is to direct attention to some of the more unsatisfactory logical features of our present thermodynamics. It seems to me that there are two main areas in which the logical situation is still unsatisfactory. There is in the first place the area of phenomena concerned with the first law and with heat as a form of motion. These phenomena were pretty well explored before the formulation of the first law, but nevertheless I believe that there were logical implications which are evident only now with our present background of quantum mechanics. In the second place, there are the fluctuation phenomena typified by the Brownian motion, not known to the

founders of thermodynamics. It seems to me that had these phenomena been known they would have left some explicit mark in the formulation of the laws.

We consider now the phenomena of the first area, the area of conservation of energy and of heat as a mode of motion. There was a long historical background in long and obstinately unsuccessful attempts to produce a perpetual motion machine. The early devices were almost entirely of a mechanical nature, and go back to the time when the laws of static equilibrium of forces were not properly appreciated. With the formulation and study of these laws it became apparent that no purely mechanical device could ever do more than keep running forever, and that if it did run forever no useful work could be got out of it. The reason that actual machines did not run forever is that they are imperfect, and this imperfection took the form of friction or inelastic collisions. When knowledge of mechanics had progressed far enough for kinetic and potential energy to be recognized, it was seen that mechanical systems conserve their energy, except for the action of the so-called imperfections, friction and inelasticity. Whereupon a conceptual invention was made, the perfect mechanical system, with no friction or other source of loss. These systems could be approached in practise, so that in practise mechanical conservation could be approached in certain types of system, but actually all mechanical systems lost energy.

The next great experimental step was the discovery that inelastic collisions and the overcoming of friction are always accompanied by the appearance of heat. It was not long before quantitative relations were being found here, a development in which our Count Rumford played such an important part, leading presently to the accurate determination of the mechanical equivalent of heat by Joule. That is, the disappearance of a definite amount of mechanical work was found to be always accompanied by the appearance of a definite amount of heat. The converse process, the disappearance of heat and the appearance of mechanical energy, was also a process which was found to be subject to mathematical regularities. The full convertibility of heat into work was, however, subject to restrictions not applying to the direct process. These restrictions were formulated in the second law.

With the discovery of the interconvertibility of heat and mechanical work (I use "mechanical" in a general sense to include electrical, magnetic, etc.), the physicist should logically have stopped. That is, we recognize energy in two forms, thermal energy and generalized mechanical energy, and it is the two forms together which are conserved. The physicist, however, would not stop here, but as a human being had to have his explanation, which he achieved in a flash of insight by postulating that heat energy is mechanical energy of the small parts of which matter is composed. The conservation of energy thus becomes nothing but the conservation of mechanical energy, mechanical energy of the ultimate particles. However, these ultimate particles behave like no known mechanical system, but only like systems which are sometimes approached in practise when we can minimize the imperfections of friction and inelasticity. That is, a microscopic mechanical system is postulated, like no known actual mechanical system, but highly idealized. Why was the physicist so pleased with his idealization and so sure that he had the right answer? What was the experimental evidence that microscopic mechanical systems do not have the imperfections of macroscopic mechanical systems? There was, of course, no experimental evidence. The picture was so appealing because it was consistent with a long-standing picture of matter as consisting of ultimate hard massive particles with no internal structure and completely described by their mass, position, and velocity. The argument for the analysis of matter into such particles is a purely metaphysical argument, somehow deeply rooted and congenial to human habits of thought, and going back to at least the time of Lucretius.

The assumption of particles resolves as it were by fiat a dilemma which the physicists of the time were not sophisticated enough to see, but which later did bedevil physics for years. For if matter is not composed ultimately of discrete particles but is truly continuous, and if the laws of mechanics apply to such continuous matter, then why do not all macroscopic motions dissipate themselves by burrowing deeper and deeper until they finally disappear? This is the same catastrophe that was later known as the ultraviolet catastrophe in connection with the luminiferous ether, which was assumed to be truly continuous. The ultraviolet

catastrophe was avoided by postulating the discrete character of radiation demanded by quantum theory. Logically the postulation of massive particles with no internal degrees of freedom would seem to be much akin to quantum theory, but I believe that we cannot, for that reason, absolve either Lucretius or Rumford from having been pretty metaphysical.

There are other respects in which the classical formulation of the first law is at loose ends logically. It is recognized that energy can be imparted to a body in two forms, as mechanical energy in the form of mechanical work, and as thermal energy in the form of heat. Now although it is assumed that the distinction between heat and work is possible, no directions are usually given as to how the distinction is to be made in the laboratory, but the matter is left to the instinct of the experimenter. It proves that usually the instinct of the experimenter is safe enough, but nevertheless it is not difficult to embarrass him if one makes the effort. In order to escape embarrassment the experimenter usually arranges matters so that he deals only with heat without mechanical work or with mechanical work without heat, for it is only when the two occur together that difficulties arise. But it is not always possible to separate heat and work with the instruments of the laboratory, and we are forced to deal with the situation conceptually. This we do by saying that we still have conservation in such cases, and that although we can no longer distinguish a stream of thermal energy and a stream of mechanical energy entering the body, the two streams merge into a stream of generalized energy. No instrument responds to this stream of generalized energy, the meaning of which must be sought entirely at the paper and pencil level in the doings of the ultimate particles and in the microscopic electromagnetic field. The necessity of recognizing a stream of generalized energy does not usually embarrass us, because in practise we can usually find methods of changing the state of the body under circumstances such that work and heat are defined, and we can then play the game backward and calculate mathematically what the stream of generalized energy must have been, in terms of its divergence, which is given by experiment. But if the body undergoes changes which intrinsically can never be analyzed into heat and work, and we have no way of knowing at present that there may not be such situations, then

it is not known whether conservation is true or even whether energy has meaning.

There is one comparatively minor matter in the classical analysis of energy into heat and work which I think has never been satisfactorily formulated. It is usually assumed that the work and heat received by systems may be evaluated by determining the work and the heat given to them by the external universe. This treatment fails in the case of friction, because the external universe delivers mechanical work which the system receives as heat. The difficulty would seem to be connected with the surface of discontinuity on which friction takes place. The difficulty here is only formal, although it is seldom recognized, and I believe can be surmounted by a special formulation when systems of discontinuity occur.

We now consider the second area of thermodynamic phenomena, the area of fluctuation phenomena, which were not known when the laws of thermodynamics were formulated. The logical dilemma thus thrust upon us is obvious, because it appears that the fundamental physical assumption used in defining temperature does not correspond to the facts. Temperature is defined in terms of equilibrium states, but if we make our instruments delicate enough we find that equilibrium never occurs. The smooth, continuously variable states assumed in classical thermodynamics are only an approximation, which in the early days appeared to be an approximation that we could approach as closely as we pleased, but which we now see recedes from us as we make our instruments as fine as we have now found how to do. The situation is like that presented by a semi-convergent series in mathematics. The universe of thermodynamics is the universe of the flat plateau of apparent convergence.

With the appearance of fluctuation phenomena all sorts of questions arise. Can we formulate the restrictions to which our operations must be subject if we want to be sure of remaining on the plateau of thermodynamics? The original expectation was that the universe of operations of thermodynamics included all the operations that we were capable of performing physically. Now we see that there must be restrictions, but what are they? How small is it permissible to make our instruments if the results we find are to satisfy thermodynamics? or how long must we

wait for equilibrium before thermodynamics will give us an approximate answer? Practical questions arise. Is it possible by ingenious enough construction to violate the second law of thermodynamics on an industrially profitable scale? The answer is not obvious, and the invention of his demon by Maxwell has raised genuine doubts in many minds. The answer is not to be found in the completely random character of the small-scale fluctuations, for it is possible on a larger scale to extract useful energy from the completely random motion of the waves of the sea, for example. This can be done by the use of a ratchet mechanism. We have to ask why a ratchet mechanism is impossible on a scale small enough to deal with fluctuations. It is no answer to say that the ratchet would be subject to fluctuations, and so could not function, until we have shown how the different sorts of phenomena, such as the optical and the tactual, leave the plateau together as we progress toward the microscopic. But to show this would be pretty nearly equivalent to assuming what we would like to prove.

The average physicist reacts to these considerations by admitting that thermodynamics is a tool of limited applicability, which must be replaced by the more powerful tool of statistical mechanics if one wants really to understand microscopic phenomena. At first it was the statistical mechanics based on classical mechanics, but now this has to be supplemented by quantum mechanics, in which the notion of probability is brought in at the beginning, as an intuitive concept, not to be explained. I believe there can be no question but that statistical mechanics is a satisfactory and powerful tool in providing answers to concrete problems; our concern here is rather with its logical status. Consider first classical statistical mechanics. The fundamental laws here are those of Newtonian mechanics applied to a system of particles. But a hard particle, with only mass, position, and velocity, is a construction for which no logical justification whatever can be given. In assuming that energy does not reside inside the elementary units we have by fiat sidestepped the whole logical difficulty. Because of this, classical statistical mechanics in no wise has the character of an explanation in terms of which we can understand, but rather it must be regarded as a discovery, which enables us to compute correct answers, but which does

not afford the understanding for which we had hoped. One of the recognized difficulties of classical statistical mechanics is that logical considerations of probability are inapplicable to concrete situations. It has seemed to many that this difficulty is turned by quantum mechanics by introducing probability concepts into the very foundations on an intuitive basis, and by recognizing that the results are not applicable to single concrete instances, but are applicable only to ensembles. I believe, however, that the fundamental logical difficulty is not to be removed in this way, because logically actual ensembles are concrete systems, of a higher order it is true, but nevertheless concrete, and therefore incapable of admitting such operations as, for example, taking mathematical limits as is necessary in making the concrete application. Both classical and quantum statistical mechanics must be recognized to be paper and pencil devices, of which it is meaningless to ask whether the description they afford of microscopic phenomena is true or not. In general, the meaning of our concepts on the microscopic level is ultimately to be sought in operations on the macroscopic level. The reason is simply that we, for whom the meanings exist, operate on the macroscopic level. The reduction of the meanings of quantum mechanics to the macroscopic level has, I believe, not yet been successfully accomplished and is one of the major tasks ahead of quantum theory.

In the meantime the spirit of the approach of thermodynamics is from this point of view logically satisfying, because the operations of thermodynamics are macroscopic operations. We are driven, I think, by the existence of fluctuation phenomena to recognize that we can never expect, as we had at one time hoped, for a logically clean-cut scheme of unlimited physical applicability on the macroscopic level. But this, I believe, is no more than we are driven to from every other line of physical analysis, when we push the analysis as far as we can. Sharpness and freedom from fuzziness have never been attained, not even in the operations of logic itself. In the meantime, thermodynamics, with its use of large-scale operations, stands as a sort of prototype of the best that we shall ever be able to do.

In the last few years it has been appearing that we can push further with the macroscopic analysis than at one time seemed possible. It has always been a reproach to thermodynamics that

its handling of irreversible phenomena was inadequate or even impotent. We are now finding how to deal with large and important groups of irreversible phenomena by methods in the spirit of thermodynamics. For there are groups of irreversible phenomena which are adequately describable in terms of macroscopic operations, such as heat flow by conduction, or resistance heating by an electrical current, or the motion of a viscous liquid. These phenomena can now be handled by a broadening of the classical concepts of thermodynamics. These broadened concepts include the recognition of fluxes of heat and mechanical energy and generalized energy, as well as fluxes of entropy and creation functions for entropy. It is possible, I believe, to go even further in the treatment of irreversible phenomena in the spirit of thermodynamics. Classical thermodynamics defines the entropy only of those states of a body which can be reached by some reversible process from a standard state. Such a definition rules out on principle most of the matter of daily life, because most states can be subject to no reversible displacement whatever — any plastically deformed metal is an example, or any biological system. It is possible to generalize the entropy concept for some simple systems subject to essential irreversibility of this sort, and to find the specific form of the entropy function in terms of the parameters of state by imposing the requirement that the total entropy of the universe must increase for every possible method of coupling the system with the external universe and for every possible displacement. I believe that in general the analysis of such systems will be furthered by the recognition of a new type of large-scale thermodynamic parameter of state, namely the parameter of state which can be measured but not controlled. Examples are the order-disorder rearrangements in mixed crystals, measurable by X-rays, and the dislocations in a solid, measurable by the attentuation of supersonic vibrations. These parameters are measurable, but they are not controllable, which means that they are coupled to no external force variable which might provide the means of control. And not being coupled to a force variable, they cannot take part in mechanical work. Such a parameter of state, which enters into no term in the mechanical work, can be shown by simple analysis to be one which can take part only in irreversible changes. In working out the thermo-

dynamics of actual systems in which such uncontrollable parameters play a part, the first step will be the determination of the equation of state in terms of the new parameters. Such will be a matter of considerable difficulty, because the effects of such parameters are usually obscure and small. However, this is an essential step and until it is taken we cannot hope to master the thermodynamics of such systems.

The Management of Flame

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1. *Introduction.* The title of this communication is suggested by the title of Rumford's Sixth Essay: " Of the Management of Fire and the Economy of Fuel." Rumford is famous for a great many achievements, but foremost of all these was his realisation of the importance to civilisation of the right management of fire and heat. Even today full heed has not been given to his doctrine. In recent years perhaps there has been a greater urge to utilise energy with less waste, but even so the waste is far greater than the energy usefully applied.

The Management of Fire and the Economy of Fuel is not the concern of this note. That would be too vast an issue in these days! " The Management of Flame " limits discussion to the burning of gases, but it is a wide subject with many aspects. There is, however, need to manage flame whether it be for the design and control of furnaces, or for internal combustion engines or turbojet engines, or rocket motors, or just for burner flames. Up to the present flame has not been very manageable, in the sense that the exact combustion behaviour could not be predicted, under given circumstances in a particular contrivance, in the way that aerodynamic behaviour can generally be assessed through application of model laws. Efforts have been made in recent years to study more closely the combustion of gaseous mixtures with a view eventually of making the combustion processes in practise more amenable to calculation. Besides the actual chemical oxidation of the fuel there are many other steps in the process which have to be considered, such as mixing of the gaseous fuel with air, and heat transfer by conduction and radiation. With *liquid fuels,* as well as mixing, there are complicated questions of droplet distribution, penetration, evaporation, transfer of heat to the droplet for vaporisation, and mass transfer of the vapour from the droplet. Similar problems arise in disperse systems of solid combustibles. The simple combustion process in the gaseous flame itself is quite enough

of a problem to manage and it is that with which my note will mainly be concerned.

2. *The Simple Flame.* It is customary to start by considering a plane flame front, which remains stationary because the unburnt gas mixture flows into the flame zone as fast as the flame consumes it; the hot mixture of burnt gases flows away.

The burning velocity is defined as the mass of gas mixture burnt per unit area of flame surface per second. When the flame is situated as a plane at right angles to the lines of flow of the gas mixture, then this definition suffices, and as $M = V\rho$, where ρ is the density and V the volume of the gas at normal temperature and pressure, V is more often specified in the figure given for the burning velocity.

This is for premixed flames. Many flames (in fact most met with in practise) are diffusion flames (or only partly diffusion and partly premixed). A diffusion flame, like a candle flame, is one in which the fuel and the supporters have to diffuse the one into the other and the combustion is mainly dependent, not on the burning velocity, but on the mutual rate of diffusion of the gases.

We shall be concerned mainly with the premixed flame.

Consider first the plane flame:
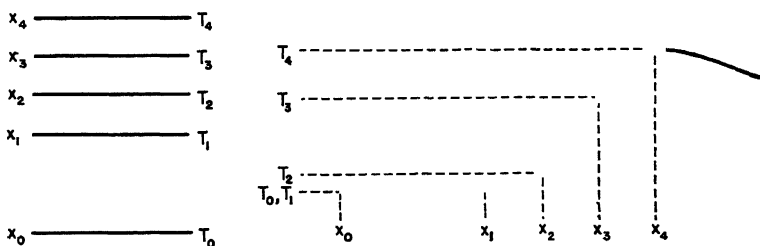
At x_0 and T_0, the gas is streaming in at the normal temperature and pressure;

At x_1 and T_1, owing to conduction of heat from the flame zone, the temperature is beginning to rise perceptibly;

At x_2 and T_2, the gas has started to react at an appreciable rate;

At x_3 and T_3, the reaction is so fast that luminous combustion is observable; and

At x_4 and T_4, the reaction is complete. Thereafter, the burnt gases start to cool.

In order to analyse a flame the change of the magnitude of one or more variables must be chosen to describe the behaviour. Temperature can be chosen, though burning velocity, density change, or extent of reaction in principle could be used. Although *inside* the flame, temperature equilibrium may not have time to be established in some positions, yet in each such position or plane there must be a temperature assignment. The following equation is a statement of all the possible variables influencing temperature for the one individual case of a plane flame front (the change in any of the variables being perpendicular to the plane flame front).

$$\frac{\partial}{\partial x} K \left(\frac{\partial T}{\partial x} \right) - M \frac{\partial}{\partial x} \left(TC \right) + M \frac{\partial}{\partial x} \sum_n Q_n \, \epsilon_n + \frac{\partial R}{\partial x} = 0$$

K = Conductivity (varying with T and composition).

M = Mass burnt per unit area per unit time.

C = Specific heat at constant pressure.

Q_n = Heat of reaction, n = number of different reactions taking place.

ϵ = Fraction of total reaction completed at position x.

R = Radiant flux.

Integration between x_0 and x $\left[\left(\dfrac{\partial T}{\partial x} \right)_{x=0} = 0 \text{ and } \epsilon_x = 0, \ x \text{ re-} \right.$ ferring to unburnt gas $\Big]$

gives: $K \dfrac{\partial T}{\partial x} + M \sum_n Q_n \ \epsilon_n = M \left(TC - T_0 C_0 \right),$

for $(R\text{-}R_0)$ is a minor factor in the preignition zone and can be neglected.

Even in this simple form[1], the equation does not lead to an estimate of the burning velocity from a knowledge of the temperature in any position x_1, for K and C vary with temperature and with the amount of reaction. This latter quantity depends on knowledge of the rate of the reactions; it is only permissible to write $Q\epsilon$ for $\sum_n Q_n \, \epsilon_n$ if each participating reaction through each layer is completed to a proportionate extent; no spatial separation of component reactions must occur. If, however, accurate means

of measuring the burning velocity and the temperature parameter through the various flame zones can be found, the application of methods of successive approximation can be applied to make the equation yield values of Q_ϵ, the extent of reaction as measured by the heat contributed. The enthalpy distribution in the flame can so be found. Beyond the flame front there is flow of energy by conduction back against the direction of average material flow. This flow becomes greater, the greater the release of energy and the higher the temperature of the particular region of the flame. Eventually in spite of still rising temperature, the rate of supply of energy diminishes, but the loss by conduction continues to increase, so somewhere in the flame there is an enthalpy hump. It is of considerable interest to determine the position of the hump, for this will provide information as to conditions of temperature equilibrium in the flame. An accurate knowledge of the temperature distribution from layer to layer and of the extent of the reaction can then be used to compare with figures derived from the measured kinetics of the homogeneous reactions at temperatures encountered in the flame zones, and so to discover whether the heat transfer (by conduction) from the flame into the unburnt gas or the mass transfer (by diffusion of radicals) from the flame is of most consequence in maintaining the propagation. Both processes no doubt play a part.

Up to the present, there has been little precise knowledge of the structure of a simple flame. This has been partly due to the great difficulty of obtaining a suitable flame on which to carry out measurements.

3. *The Flat Flame.* During some work on the determination of limits of combustion,[2, 4] Dr. J. Powling and I devised a novel type of burner which gave an even distribution of velocity of flow across its wide mouth, and provided the conditions for establishing a plane flame front, such as that discussed in section 2. A matrix formed of plane strip wound along with a corrugated strip of cupronickel provides numerous evenly spaced channels in which the flow is streamline. The flame takes up a position above the rim of the burner and is a thin circular disc. Generally there is a small turn up or turn down at the edge extending over a distance of about a millimetre, but this is small compared with

the diameter of the disc which can be made as large as is convenient (for instance, 8 cms.).

A gauze, situated usually about 4 or 5 centimetres above the flame, not only prevents convection currents disturbing the flow conditions, but helps to reflect heat back to the matrix and this preheat (usually about 100°) is essential for the stability of the flame.

With heat provided in this way, stability is only obtained for slow flames (of mean velocity 3 to at most 15 cms/sec). Nevertheless, measurements of burning velocity near the limits of combustion are being obtained by this method[3] which promise good accuracy. Incidentally, the flame can be established and maintained for mixtures considerably weaker than the usually accepted limits mixtures.

Recently[1] this flame has been used by Mr. Weinberg in our laboratory at the Imperial College, South Kensington, to effect a determination of the temperature parameter throughout an ethylene-air flame by an optical method depending on the change of refractive index of the gases, and some progress is being made towards the study of the structure of the simple flame outlined in section 2.

4. With the flat flame Mr. Badami and I have been determining the burning velocities of mixtures of air with hydrocarbons from methane to butane, and now with mixtures of gases such as hydrocarbons with hydrogen and with carbon monoxide. It is interesting that the plane disc-like flame breaks up into the cellular structure under certain circumstances. These cellular flames have long been known, but they have been more closely studied recently by Markstein.[5] Using the flat flame burner, it is possible to study the change over from the flat flame to the cellular combustion. For instance, with a 1.9% ethylene-air mixture to which hydrogen is added, a change from 3.14 to 3.15% hydrogen will cause a small tip to form near the edge of the flame which on addition of a trace more hydrogen becomes a single cell directed towards the tip of the burner which proceeds to rotate round the flame edge — the sense of rotation can be changed by slightly altering the tilt of the burner. A trace more hydrogen causes the whole disc to break up into cells. With hydrogen and carbon monoxide mixtures we

have found that not only enriching with hydrogen, but also enriching with carbon monoxide produces the change from a flat flame to a cellular flame. It is possible that the instability is not necessarily associated with an increase in hydrogen atom concentration, although observations in general have been in accord with such a hypothesis.

The maintenance of a stationary flat flame is dependent on the balancing of the buoyancy of the gas layers against the force of gravity. There is a pressure difference across the luminous flame front, though very small. A perturbation can arise and can become a permanent state, breaking up the front into cells. The flat flame can only stabilise itself if lateral expansion can occur and this will probably only be possible, not only if the preheat is such that sufficient burning can occur at the existing flame front, but if certain diffusion conditions are also satisfied. The conditions of stability of the flame have yet to be clarified. Vibratory conditions often arise in flames, more often in closed vessels, in which pressure waves reflected back are shown to cause disturbance of the conditions in the flame front. It is however interesting that a flame can self-generate fluctuations and these cellular flames will repay further study.

5. It is a difficult matter to investigate the fast chemical reactions which occur in flames. Several methods have been used:

(a) *Sampling*, particularly using the mass spectrograph.

(b) *Spectroscopic* methods.

(c) *Sudden condensation.*

Our laboratory at South Kensington has employed the last two methods. Dr. A. G. Gaydon's spectroscopic researches on flames, and particularly flames at low pressure, are well known.[6] He has shown that OH, CH and C_2 radicals are sometimes in exceptional states of high rotational energy and that some substances introduced into flames reach high levels of excitation. He has mapped the region of appearance of these radicals and this has led to knowledge of some of the reactions which provide radicals such as CH and C_2. It is clear that in premixed flames thermal equilibrium is often not complete amongst the products. Under certain conditions in hydrocarbon flames, the CHO radical shows up prominently and is generally associated with peroxidation.

Method (c), sudden condensation, has been used by Dr. Minkoff and Mr. Everett,[7] who have shown that quite considerable quantities of peroxides are formed in hydrogen-oxygen and methane-oxygen explosions at low pressure. These explosions at low pressure have been photographed. They take place at great speed, but in spite of that, from the yield of peroxide under different circumstances, it is possible to find out something about the reactions which occur in these fast flames. Hydrogen and oxygen explosions yield hydrogen peroxides of two kinds, the ordinary and one which appears to be unusually unstable and breaks up on warming to ordinary temperature. Methane-oxygen explosions yield some ozone and also some methyl hydrogen peroxide in addition to the hydrogen peroxide. The ozone probably arises from the combustion of the CO formed during the combustion of the methane. It is not yet known whether the methyl hydrogen peroxide (which is only about 0.1% of the total peroxide) is a direct product of the oxidation or is only the result of subsidiary reactions.

Whatever reactions do occur when once the luminous boundary of the flame is passed, they occur with very great rapidity. The thickness of even a quite slow moving flat flame is a few tenths of a millimetre and for a large molecule like octane, much has to happen in a very short time.

It is possible to estimate for the flat flame how long there is between the moment when the temperature begins to rise ahead of the flame and the moment when luminous combustion first shows. The time is very small and in the case of methane it seems to be less than the delay for ignition of a like mixture of methane and air at 1,000°C. That the flame is maintained merely by conduction of heat into the unburnt gases does not therefore seem probable. There are good grounds for believing that radicals (hydrogen atoms chiefly) are also diffusing back to assist the initiation of the chain reactions.

6. In the above paragraphs, some of the ways have been mentioned in which simple premixed flames are being investigated. Parallel with this, the diffusion flame has been closely studied. Dr. Barr (University of Glasgow) and Drs. Parker and Wolfhard (Royal Aircraft Establishment, Farnborough) have made some interesting investigations. Dr. Barr has investigated the conditions controlling

the length of simple diffusion flames of butane in air over a very wide range of flows. Vibrating flames which are set up under some conditions appear to be due to enhancement of fluctuations in the fuel flow and not to a change in the nature of the chemical reaction in the flame.

Dr. Wolfhard[8] devised a diffusion burner of a special type which has made it possible to map closely by spectroscopic means the various radicals produced as the reaction proceeds in the various interdiffusing regions of the flame, and to relate this to the temperature distribution.

Mr. Rudrakanchana in my laboratory has been using the method to study the diffusion flames of organo-metallic substances. The object was to investigate the production of the solid products; the polymerisation to sizable particles. Just as with the formation of carbon particles in flames, the period is very brief. It is remarkable how quickly a solid product, such as carbon or lead oxide formed in a vapour phase chemical reaction, will even at great dilution polymerise to a solid particle in a disperse state. It is easily shown by the flat flame that the time available for carbon to form and then polymerise is only about one millisecond.

7. As mentioned earlier, in practise diffusion flames are often as important as premixed flames. In a spray of liquid particles heat has to be conveyed from particle to particle, and vapour to be distributed in air. If the particle is small enough, and mixing is rapid enough, the combustion behaviour may approximate to a premixed flame, but if the particles are larger or the mixing not rapid enough, the combustion behaviour will approximate to that of a collection of diffusion flames. The behaviour will be complicated by turbulence. Even these conditions are yielding to analysis.

8. I have attempted in this communication to indicate some of the directions in which investigations are proceeding on simple flames. Much valuable investigation is going on in many laboratories in other countries than my own and if I have not referred to such work, I plead the brevity of this report.

The object of all the work is for the better management of flame; in other words, in order to obtain such knowledge of flame behaviour that furnaces and other contrivances may be designed

so as to operate as required. We agree with Count Rumford that "the advantages that may be derived from the Knowledge" of the "communications of Heat by flame are of very great importance and the subject deserves to be thoroughly investigated."

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Helium and the Range of Stability
of the Solid State

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Much is known about the vapour-pressure curves which deter-
mine the gas-liquid and gas-solid boundaries in the equilibrium
diagram (Figure 1), but of the melting-curve which defines the
liquid-solid transition little can yet be said. Why is this?
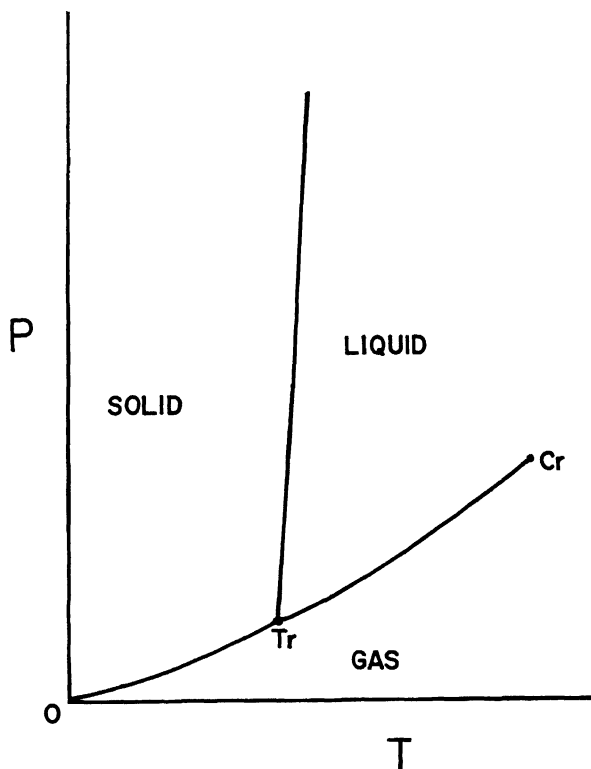
FIGURE 1. SCHEMATIC EQUILIBRIUM DIAGRAM OF THE THREE STATES OF
AGGREGATION.

We can of course apply the Clausius-Clapeyron equation to both cases:

$$\frac{dp}{dT} = \frac{Q}{T\Delta V} \tag{1}$$

($Q =$ heat of evaporation or melting, $\Delta V =$ volume difference between the phases). For the two vapour-pressure curves we can — at least at the lower temperature end — replace ΔV simply by the volume of the ideal gas. In addition, we have quite a clear conception of the heat of evaporation and we also know that the temperature dependence of this quantity is not very strong, at least up to the normal boiling point. Letting this dependence vanish in first approximation, we arrive at the well-known equation:

$$\ln p = -\frac{Q}{RT} + const. \tag{2}$$

In the case of the melting curve, however, both phases are in a very dense state and no obvious "ideal" laws can be applied. No general explanation of the volume change or of the heat of melting can be proffered, nor can we predict their dependence on temperature; consequently, it is not possible to integrate the Clausius-Clapeyron equation. We can only say that the melting curve must always be extremely steep because of the very small volume difference between the two phases of aggregation. In fact, even the highest pressures which Bridgman[1] could apply have, in the case of the substances melting at normal temperatures, failed to raise the melting temperature much above the temperature of the ordinary gas-liquid critical point.

Without further theoretical guidance most observers have in these circumstances rightly refrained from attempting to generalize their results with the exception of Tammann,[2] who fell into the familiar trap of extrapolating interpolation formulae valid over only a very limited range. His prediction that the melting temperature would rise to a maximum and then fall with rising pressures, so that the solid state would form a finite enclave, has, however, unfortunately dominated the ideas of some geophysicists for a long time.

A knowledge of the behaviour of the melting curve at very high pressures and temperatures is not only desirable because of its intrinsic interest for the condensed state but also for geophysical applications. I therefore tried some time ago to find out

whether the parameters of the melting curve could not be related to some general physical property of either of the phases. In order to do this I tried first to find out[3, 4] from the experimental material — which we owe mainly to Bridgman — how a suitable dimensionless quantity changes along the melting curve. The quantity $\frac{r}{p\Delta V} = \frac{d \ln p}{d \ln T}$ ($r =$ heat of melting) was chosen and all the known melting data were plotted in a $log\ p - log\ T$ diagram. It then turned out that all melting curves become straight lines at the higher temperatures and that in addition most of them are parallel to each other. Naturally, in the region of the normal melting point — that means at very small pressure — the linear correlation must break down, as the log of the pressure must approach minus infinity at a finite melting temperature. This, however, is only to be expected. The pressure zero does not command a position of particular significance in condensed systems, in contrast to gases, and even negative pressures are perfectly possible because of the cohesion of condensed phases. It was then tried whether the simple addition of a term representing an appropriate internal pressure (a) would improve matters. Indeed it was then found that for every substance an a could be chosen which would render the relationship between $log\ (p + a)$ and $log\ T$ linear. Thus the formula:

$$log\ (p + a) = c\ log\ T + b \qquad (3)$$

resulted or, equivalently,

$$\frac{p}{a} = \left(\frac{T}{T_0}\right)^c - 1 \qquad (4)$$

($T_0 =$ normal melting temperature). Furthermore, it was found that the a's determined in this way agreed very well with the internal pressures calculated as $\left(\dfrac{\partial U}{\partial V}\right)_T$ using van der Waals' equation. As mentioned before, the curves were nearly parallel, which means that the constant c has about the same value (about 2) for all substances except the metals.

These features gave us sufficient confidence to use this formula in planning our experiments. Given a certain limit of physical pressure, equation (4) predicts that one should employ a substance of small internal pressure in order to penetrate to relatively

high pressures and temperatures. Now the internal pressures of the substances melting at ordinary temperatures are of the order of 5,000 atmospheres, while substances of low internal pressure will be those with weak interatomic forces, that is, substances having low boiling points. Thus from van der Waals' equation one can estimate that for helium a should be of the order of 20 atmospheres. This means that by using helium instead of a substance melting at normal temperature, we would have in our favour a factor of 250 in the pressure. Experiments[5] on helium soon showed that even at the boiling point of hydrogen, i.e., at four times the critical temperature of helium, it could be solidified under rather moderate pressures (2000 atmospheres), and in later experiments the author and his collaborators extended these measurements to 40° abs.[6]

Of course, in considering helium, one must take account of the anomalous behaviour of this substance at very low temperatures: helium does not solidify at all under its saturation pressure and therefore has no triple point. However, the change over from normal to anomalous behaviour takes place very abruptly at the λ-point*; the melting curve at higher temperatures can be regarded as a curve for an ordinary substance with a normal melting temperature of about 1° abs.

Apart from helium and hydrogen, neon, nitrogen, and argon were also investigated[8] and Table I shows that in accordance with the prediction of equation (4), a given external pressure (5,000 atmospheres) raises the melting point $T_{5,000}$ to a *relative* temperature $(T_{5,000}/T_{cr})$ which is the higher the lower the critical temperature (T_{cr}) of the substance.

TABLE I

	T_{cr}	$T_{5,000}/T_{cr}$
He	5	7.3
H$_2$	33	2.3
Ne	45	1.7
N$_2$	126	1.0
C$_6$H$_6$	563	.7

*See Simon and Swenson.[7] In this paper we also discuss the significance of the melting curve of helium near absolute zero.

Thus the semi-empirical formula was quite successful and, in fact, even workers who did not believe in the significance of the internal pressure term used it to represent their experimental results.

New Experiments

In the last few years we have started up work again in this field. We have made further experiments and have investigated in more detail both the meaning of the reduced formula and the extent to which we can use helium as a model substance. We recall that in the last few years attempts have been made by Domb[9] and de Boer[10] to derive the melting formula from first principles. Using the approach of Lennard-Jones and Devonshire,[11] they have succeeded in doing this for the region of high temperatures and pressures; in particular de Boer was able to show that the constant a really corresponds to the internal pressure. The model used by these authors had of necessity to be oversimplified and therefore it was not possible to derive an accurate expression for the constant c. Also this model could not really be applied to a degenerate substance like helium.

Very recently a more general derivation of equation (4) has been given by L. S. Salter[12] of the Clarendon Laboratory. As we mention later on, our new experiments show that the Lindemann melting point formula is valid for solid helium over a very considerable range of temperatures and pressures. Salter combined the Lindemann formula with Grueneisen's equation of state, and showed that the melting formula follows immediately and that a corresponds strictly to the internal pressure. This deduction is, moreover, not restricted to the classical region. Although solid helium is in a highly degenerate state and although zero point energy plays a very big role in both the solid and liquid phases, Salter's treatment, together with the previous experimental knowledge that the melting curve for helium can be reproduced over the whole range by formula (4), is a justification for using helium as a model substance for the process of melting. (The reason why helium obeys a reduced formula in spite of quantum effects is that their main influence is on the internal pressure, and the parameter a automatically takes care of this. If, however, we follow de Boer[18] and use molecular constants as reducing parameters,

then large deviations from the law of corresponding states will occur at low temperatures.)

Before describing the new experiments, a word about solid helium which may appear to some as a rather unusual solid. Solid helium is a very hard substance which can block capillaries against thousands of atmospheres of pressure difference. Keesom and Taconis[13] have shown that it has a normal type of close packed hexagonal structure and we have even observed a crystallographic transition in it. (See below.) The melting transition is very sharp and the liquid in equilibrium with the solid possesses a very low viscosity.[14] Webb and Wilks[15] have shown that solid helium has a thermal conductivity like that of other normal crystals and that the helium generally solidifies as a single crystal.

Our new experiments on the melting curve of helium were concerned with two particular problems. Firstly, we wished to extend the melting curve to higher temperatures and pressures. By adopting different techniques from those used by Simon, Ruhemann and Edwards,[6] and by Holland, Huggill and Jones,[16] Dr. Robinson[14] has succeeded in carrying out experiments with pressures up to about 10,000 atmospheres, when helium melts at about 60° abs. (Figure 2). Similar experiments were also done with argon, the melting point of which is raised by a pressure of 10,000 atmospheres to 240° abs. As in the case of helium these experiments give good confirmation of equation (4). On extrapolating this equation one finds that helium at room temperature would be a solid at pressures above 100,000 atmospheres. This, of course, only holds if a solid-fluid critical-point does not intervene before room temperature is reached.

The second problem was to find whether there is any trend along the melting curve towards such a critical point. In order to do this we measured the entropies of solid and fluid helium along the curve; an approach to a critical point would show itself in the fact that the entropies of both phases tend to become equal. It would be extremely difficult, if not impossible, to measure the specific heats of normal substances at high pressures as the heat capacity of the high pressure container would overshadow everything else. Here again the use of helium as a model substance has a decisive advantage. At the temperatures concerned, steel has a specific heat which is quite negligible and we are able to work more or less
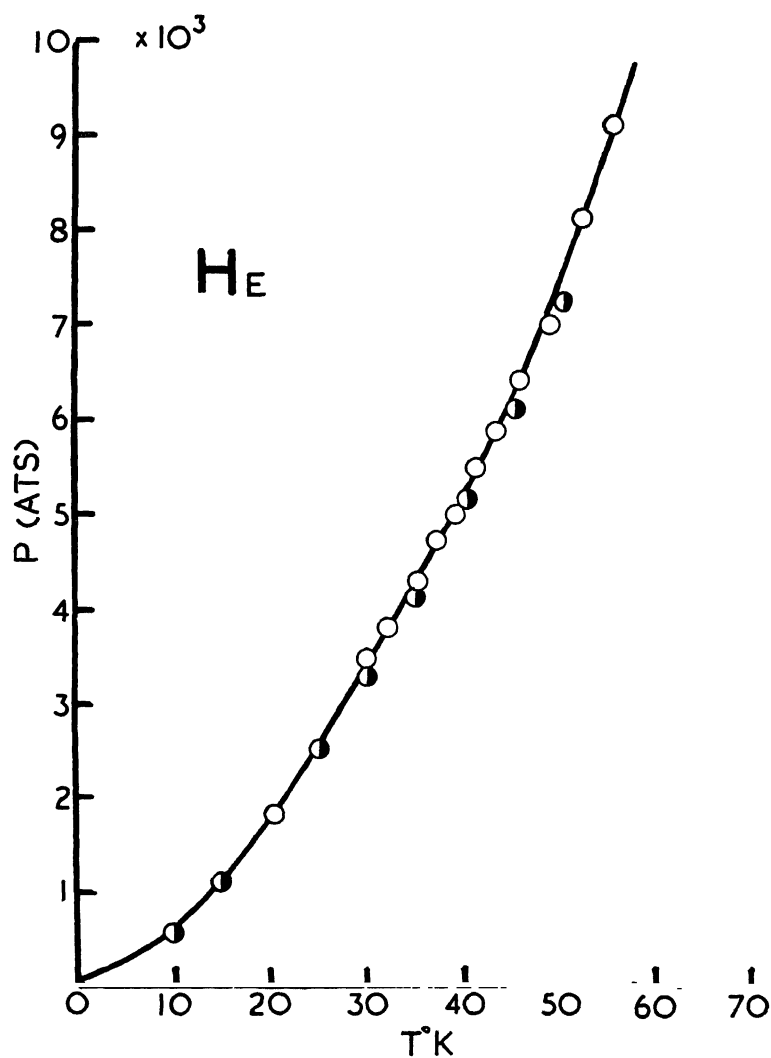
FIGURE 2. MELTING CURVE OF HELIUM.
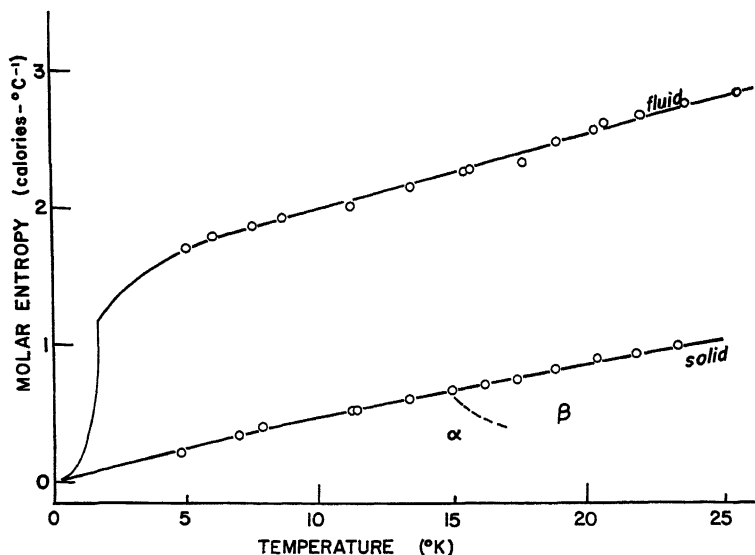
FIGURE 3. ENTROPY OF SOLID AND FLUID HELIUM ALONG THE MELTING CURVES.

with mathematical walls. It was therefore possible for Dr. Dugdale to measure the specific heats of highly compressed solid helium and the heats of melting. By applying the third law of thermodynamics one can then construct the whole entropy diagram which is reproduced in Figure 3. These experiments have so far been carried out up to 3,000 atmospheres corresponding to temperatures slightly below 30° abs. Details of the experiments, together with data on the equation of state of solid helium, will be published elsewhere;[17] here I will only mention those points that concern the melting curve.

The entropy of melting turns out to be of the order of the gas constant R, as with most other substances. The fact of greatest interest to us, however, is that the entropy differences do not diminish but rather increase slightly with rising temperature. It should be noted that Figure 3 gives data for temperatures five times greater than the critical temperature, a region in which it has not been possible to measure the properties of any other substance. From the figure it also follows that the heats of melting, in the range of temperature investigated, are about three times higher

than the heat of evaporation of liquid helium at absolute zero. This again emphasizes the fact that the differences between the two phases are very pronounced and that the difference is increasing with temperature.

As we see in Figure 3 there is a transition of the first order within the solid state. As it is impossible to see how crystalline helium could exist in any arrangement other than a close packed one and as we know that the low temperature (a) phase is hexagonal close packed, we have to conclude that the high temperature or β phase is cubic close packed. As would be expected from this explanation, the differences in energy and volume are very small and the break in the melting curve is quite invisible.

The Lindemann Formula

The volume of solid helium at 3,000 atmospheres is only 40% of its value at absolute zero. The characteristic temperature at this volume, derived from the specific heat, is about five times higher than its value for helium under the melting pressure at absolute zero. We are therefore in a good position to test the Lindemann melting point formula which connects the melting temperature with the Debye characteristic temperature θ, the molar volume and the mass of the atom:

$$\theta = const. \sqrt{\frac{T_m}{M V^{2/3}}}$$

The following table gives the data for a few selected points:

T_m	V	θ	$const.$
23.3	10.6	110	101
17.3	11.6	92	100
11.3	13.1	72	102
7.9	14.4	55	95
3.1	18.3	32	96

We see that the constant in the formula changes only by a few per cent in spite of the very great range of the other parameters. This is a rather surprising result which has been discussed in more detail by Domb.[18] The main interest for us at the moment is that it gives a justification for Salter's derivation of formula (4).

Conclusions

Summing up the results, we should mention several points. First, it has been possible to observe melting at temperatures over fifty times the normal melting temperature. Since substances other than helium agree well with the reduced melting formula, we may therefore generalize this statement to cover all substances with similar binding forces. The usual equilibrium diagram shown in Figure 1 represents the data from a much too restricted view;
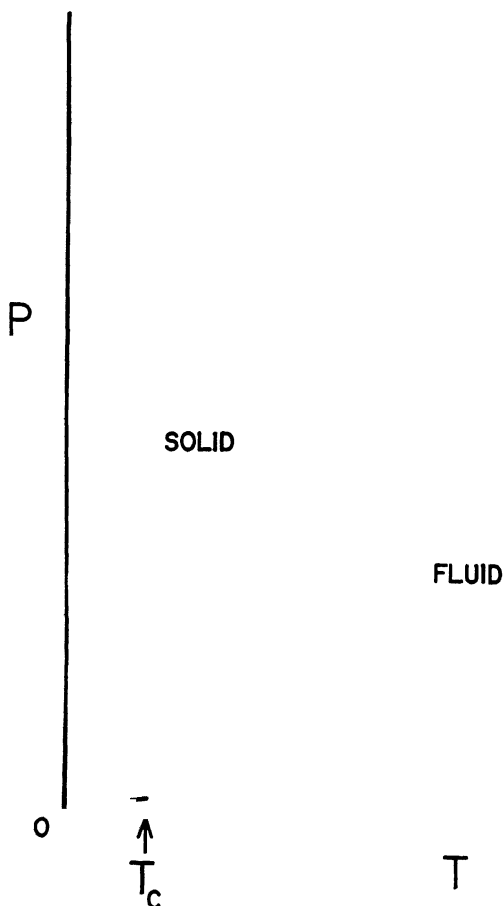
FIGURE 4. EQUILIBRIUM DIAGRAM BETWEEN SOLID AND FLUID STATES.

Figure 4 gives the diagram in a new perspective. The real dividing line is that between the crystalline phase and the fluid. The liquid-gas equilibrium is really only a very small appendix which has so far been over-emphasized because we happen to live under practically zero pressure.

The second point is that even at these very high temperatures there are no indications of our approaching a critical point. Of course, if we were to use pressures of a few hundred thousand atmospheres, then the outer electrons of the atoms would be squeezed off, and it is not possible to foresee how this would influence the states of aggregation.

Third, as we see in Figure 3, the entropy of the solid increases continuously along the melting curve with rising temperature. We had previously deduced this fact from some calculations on the alkali metals,[4] but these data did not have the same degree of certainty as have the direct measurements on helium. That the entropy curve along the melting line increases with rising pressure is not a matter of course; from the thermodynamic point of view it could just as well fall. The fact that it rises means that the effect of temperature in increasing the entropy is bigger than that of rising pressure which reduces the entropy.

This fact has one important consequence in that it shows the impossibility of melting a substance by adiabatic reversible compression. If we move along an isentropic line to higher temperatures, we move more and more into the range of stability of the solid phase. The question arises whether this is a general property of matter or only of the particular substances mentioned above. Salter's work can tell us something about this. We find that the entropy along the melting curve will always rise with increasing temperature (i.e., T/θ increases) if the quantity γ, which is defined by

$$\gamma = -\frac{d \ln \theta}{d \ln V} = -\left(\frac{\partial \ln T}{\partial \ln V}\right)_S$$

is bigger than $2/3$. For actual crystals γ has values between 1.5 and 2.5; the value $2/3$ would actually correspond to an ideal gas. The statement that one cannot melt a crystal by adiabatic reversible compression seems therefore to have a wide range of validity. It is true that one cannot make predictions for some abnormal substances like those possessing a very loosely packed lattice, but,

as Bridgman has demonstrated, even these substances are forced into more normal behaviour by high pressures.

I hope to have shown that "model" experiments with the low boiling substances can give information of general importance that cannot be obtained easily in any other way. We are now trying to extend our experiments to appreciably higher pressures, as well as to some other low boiling substances, in particular to hydrogen and its isotopes and to solutions of helium with hydrogen.

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Positronium

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It is rather striking that the experiments which have attracted the greatest attention of physicists in recent years are largely of two extreme types: they deal either with very high or with very low energies. Scattering experiments with almost all known particles are constantly extended to higher energies. Some of the important results obtained are described in the summary of the paper by Dr. Fermi. On the other hand the technique of radiofrequency spectroscopy is applied to transitions between nearly degenerate atomic levels. Some of the most beautiful and significant experiments of this type deal with the fine- and hyperfine structure of hydrogen discussed by Dr. Lamb and Dr. Purcell.

The reason for this dual interest in very high and very low energies is our increasing concern with the details of the forces between " elementary " particles, especially at short distances. If short-range forces are to have an important effect on the outcome of scattering experiments, the particles must have sufficient energy so that their deBroglie wavelength is comparable with the range of interest. Alternatively low energy experiments can be carried out with bound systems and may reveal the small remaining contribution at short range since they permit very high precision, especially if conditions are so chosen that the longer range interaction cancels in the result.

A detailed study of the interaction between two electrons is of special interest since according to present theory it should involve only electromagnetic phenomena which are believed to be fully understood, at least in principle. Electron scattering experiments at high energies are difficult since at the extreme velocities required by the low mass of the particles billion-volt energies involve a relative motion of the two electrons characteristic only of million-volt phenomena. On the other hand the observation of interelectron forces in atomic spectra is always perturbed by the dominant effect of the nucleus. For this reason we have undertaken

the study of the bound state of two electrons — one positron and one negatron — which does not contain a nucleus.*

The possibility of a bound positron-electron system analogous to the hydrogen atom seems to have been first discussed in the literature by Mohorovicic (1934), who suggested that its optical emission lines might be observed in stellar spectra. Ruark (1945) suggested the name " positronium," now universally applied to this structure. Wheeler (1946) analyzed some of the properties of positronium and of systems consisting of several positive and negative electrons. More detailed theoretical considerations have been presented, e.g. by Pirenne (1947), Berestetzki (1949), Ferrell (1951).

The gross structure of positronium states is exactly like that of hydrogen except for the effect of the different reduced mass of the electron, which reduces all energies by a factor 2 compared with a hydrogenic atom with an infinitely heavy nucleus. Thus the energy of the n'th state is $E_n = - e^4 m_0 / 2 \hbar^2 n^2$; the ionization potential is 6.8 volts, the energy of the first excited state is 5.1 ev, the Lyman a line has a wavelength of about 2,400 A, etc. The electron wave functions in the center-of-mass system are, in this approximation, the same as for hydrogen. The negatron-positron distance, which is usually the relevant coordinate, is, however, twice as large as the electron-proton distance; hence the statement that the positronium Bohr radius is twice as large as the corresponding hydrogen radius.

The symmetry arising from the equality of the masses of the two particles does not impose any conditions connected with the Pauli exclusion principle since the particles are distinguishable. The transition probability for optical (dipole) transitions is half as great as for the corresponding hydrogen lines. The transition dipole moment $<ex>$ is twice as large, the emitted frequency v is half as great, and the transition rate is proportional to $<ex>^2 v^3$.

These features of the gross structure are fairly obvious and there can hardly be any doubt that the optical spectrum of positronium will be observed in accordance with these predictions when favorable experimental conditions can be found.

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*Following the accepted practice, we shall refer to positively and negatively charged electrons as positrons and negatrons, respectively.

Of greater fundamental interest is the fine structure of the levels. This has been studied theoretically by Pirenne (1947), Berestetzki (1949) and Ferrell (1951). Karplus and Klein (1952) have investigated the effects of radiative corrections to the fine structure. The main differences between the fine structure of positronium and that of hydrogen are the following: 1. There is a relativistic orbit-orbit interaction between the particles which is negligible in the case of the slow-moving proton. 2. The magnetic spin-spin interaction between the particles is of the same order as the fine structure while in hydrogen this hyperfine structure is smaller in the ratio of the magnetic moments of proton and electron. It is thus appropriate to speak of triplet (ortho) and singlet (para) states of positronium. 3. There is an additional spin-dependent interaction arising from the possibility of virtual annihilation and re-creation of the pair. This can take place only when the particles coincide and has therefore the character of a short-range force which is of importance in S-states only. Since energy is not conserved in the virtual intermediate state, one-quantum annihilation which can conserve angular momentum in the $^3S_1$ state yields the most important contribution. This " annihilation force " has also been considered by Bhabha (1936) in connection with positron-negatron scattering.

Of particular interest is the splitting between the triplet and singlet components of the 1s ground state. This splitting is due to two terms. 1. The magnetic spin-spin interaction which gives rise to the Fermi-Segré term in the hyperfine structure of hydrogen contributes an energy $- 8\pi\mu^2\psi^2(o)$ to the singlet and $+ 8\pi/3\mu^2\psi^2(o)$ to the triplet state, making the total magnetic splitting $(32\pi/3)\mu^2\psi^2(o)$. Here μ is the magnetic moment of the electron and $\psi(o)$ is the magnitude of the electron wave function at the position of the positron. 2. The annihilation force contributes an energy $+ 8\pi\mu^2\psi^2(o)$ to the triplet state. Thus the total splitting of the ground state as $(56\pi/3)\mu^2\psi^2(o)$. Substituting $\psi^2(o) = 1/\pi(2a_0)^3$ and $\mu = \mu_0 = e\hbar/2mc$, we obtain for the splitting ΔW,

$$\Delta W = (7/3)\mu^2_0/a^3_0 = 8.45 \times 10^{-4}\text{eV} = 2.044 \times 10^5 \text{ Mc/sec} \quad (1)$$

Thus the ground state of positronium is split into two levels in the same manner as the ground state of the hydrogen atom. As in the case of hydrogen, radiative transitions between these states

are highly unlikely and we may expect the singlet level (para-positronium) and the triplet level (ortho-positronium) to be populated very nearly according to their statistical weights, i.e. in the ratio 1 : 3. The most fruitful result to date of the investigation of the properties of positronium has been a rather precise and only slightly indirect determination of ΔW.

Before discussing these measurements, I want to outline briefly how positronium is produced and how we detect it. The most important property of positronium in this connection is its annihilation. A positron-negatron pair even in a free state is unstable against transformation into electromagnetic radiation. The result of this annihilation transition is the disappearance of the negatron and the positron and the emission of electromagnetic radiation, with total energy $E_\gamma = 2mc^2 + E_+ + E_-$. E_+ and E_- are the energies (kinetic and potential) of positron and negatron, respectively, in excess of their rest energies.

The electromagnetic energy E can be emitted as a single quantum only if a sufficiently strong external field is present to absorb the momentum. Otherwise conservation of momentum requires the emission of at least two quanta. In the absence of special restrictions, to be discussed later, two-quantum annihilation is the most probable fate of a positron in matter. The two quanta are always emitted in opposite directions with equal energy in the coordinate system in which the center of mass of the two electrons is at rest. The kinetic energy of the two electrons just before annihilation is usually small compared with mc^2. In this case the two quanta have energies of very nearly mc^2 each and are emitted in opposite directions in the laboratory system.

· This characteristic 0.51 Mev annihilation radiation was observed very soon after the discovery of the positron, and the emission of two quanta, predominantly in opposite directions, was demonstrated by coincidence measurements.

Two-quantum annihilation is " allowed " only for an electron pair state of complete spherical symmetry, i.e. a $^1S_0$ state. In the language of classical mechanics this means that the two electrons must meet " head-on " (i.e. with zero orbital angular momentum) and with their spins " anti-parallel " (i.e. in a singlet state). When two-quantum annihilation is forbidden, three quanta are emitted. In the absence of external fields, two- and three-quantum annihila-

tion, respectively, are possible only from states of opposite parity and therefore never compete.

The probability of two-quantum annihilation was first calculated by Dirac (1930), in terms of an electron cross-section σ. For non-relativistic velocities this cross-section is:

$$\sigma = \pi r^2_0 c/v \tag{2}$$

where v is the relative velocity of negatron and positron, and r_0 is the classical electron radius. The rate of annihilation λ in a medium containing n electrons per cc. is then

$$\lambda = \sigma n v = \pi r^2_0 c n = 7.50 \times 10^{-15} n \text{ sec}^{-1} = 4.52 \times 10^9 \, \rho Z/A \text{ sec}^{-1} \tag{3}$$

ρ, Z and A are the density, atomic number, and atomic weight respectively of the medium. The mean life of a positron of non-relativistic velocity is then given by $\tau = 1/\lambda$.

When the annihilation by two-quantum emission is ruled out by the symmetry of the positron-negatron wave function, three-quantum emission is the process of lowest order possible. This is true specifically in ^3S states which contribute three quarters of all the collisions at low energy.

Three-quantum annihilation is characterized by a lower characteristic decay rate. Ore and Powell (1949) have calculated the ratio of the three-quantum to two-quantum rate and find

$$R_{3q}/R_{2q} \frac{4}{9\pi} (\pi^2 - 9) a \simeq \frac{1}{1120} \tag{4}$$

In the above discussion we have tacitly assumed the negatrons to be completely "free," i.e. we have neglected the electrostatic attraction between them and, for electrons bound in atoms, the repulsion of the nucleus for the positron. These effects depend on the positron energy but they will in general not change the order of magnitude of the result.

Equation 3 leads to life-times of the order of 10^{-9} to 10^{-10} seconds in typical solids, depending on the Coulomb effects considered. In gases at atmospheric pressure the life-time is of the order of 10^{-7} seconds; again this value has to be modified by a Coulomb correction. Whatever the exact effect of the correction, we can be sure of two characteristics of the annihilation of positrons in "free" collisions: 1. The decay rate, i.e. the reciprocal of the life-time,

must be proportional to the electron density and therefore to the gas pressure in any one gas (Eq. 3). 2. Three-quantum annihilation occurs only for about one in 370 positrons (Eq. 4). A factor of three multiplies the ratio given by Equation 4 because of the relative statistical weights of singlet and triplet collisions.

The situation is different if annihilation occurs in a bound positronium state. The electron density n (Eq. 3) is now the density in the positronium atom rather than the effective average density in the gas. This density is, of course, independent of the gas pressure and is of the same order as that in solids. Specifically, in the ground state of positronium (both ortho- and para- forms), it is $n = 1/\pi a^3$, where $a = 2a_0 = 1.06 \times 10^{-8}$ cm. a_0 is the first Bohr radius. Substituting this into Equation 3 we get for the singlet (para) state a decay rate of $\lambda_p = 0.8 \times 10^{10}$ sec^{-1}, or a mean life $\tau_p = 1.25 \times 10^{-10}$ sec. For the triplet (ortho) state Equation 4 yields $\lambda_0 = 0.7 \times 10^7$ sec^{-1} or $\tau_0 = 1.4 \times 10^{-7}$ sec. Thus, if all positrons formed positronium in a gas, we should expect to observe two components in the decay: a very short-lived one, due to the singlet state, containing one quarter of all the annihilations, and one with a mean life of 1.4×10^{-7} sec containing the remaining three quarters. This latter period should be independent of gas pressure provided the ortho-positronium is stable against collisions with gas molecules. Furthermore, three quarters of the annihilations should proceed by three-quantum emission, resulting in a continuous gamma ray spectrum. Annihilation without positronium formation results overwhelmingly in the emission of two mono-energetic 510 kev gamma rays.

In practice only some of the positrons form positronium, about 30 per cent under usual conditions. In some gases the ortho-positronium is rapidly destroyed by collisions. This is true for NO, NO$_2$, the halogens and, to a lesser extent, for oxygen. In some other gases, notably freon, the annihilation of " free " positrons proceeds so rapidly that only positrons forming ortho-positronium survive longer than about 10^{-7} sec. Figure 1, taken from a paper by the author (Deutsch 1951), shows the annihilation rate of positrons in oxygen and freon as a function of gas pressure. Oxygen shows the proportionality of decay rate with pressure, characteristic of annihilation in free collisions, while freon shows the pressure-independent decay of ortho-positronium.
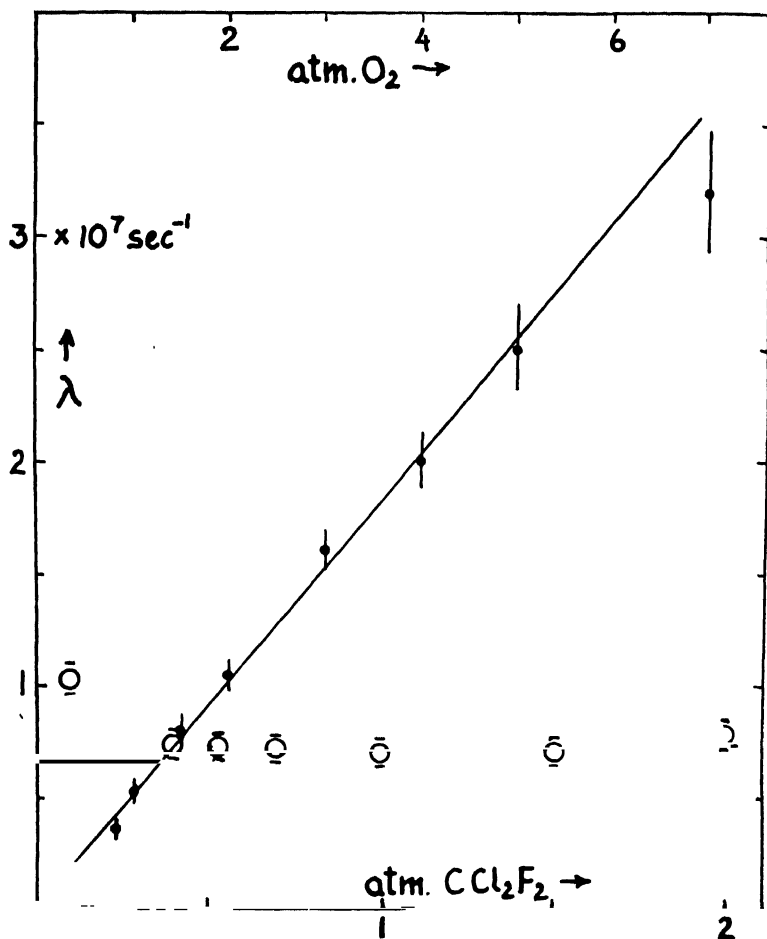
FIGURE 1

These results were obtained by measuring the time intervals be-
tween the emission of nuclear gamma rays in the radioactive decay
of Na^{22} and the emission of annihilation radiation due to positrons
annihilated in the gas. Since the positrons are emitted practically
simultaneously with the nuclear gamma ray, the distribution of
time intervals is a direct representation of the rate of annihilation
of positrons after their creation in beta decay.

Another method — in many respects more convenient — for detecting positronium is based on the measurement of the energy spectrum of the annihilation gamma rays. Two-quantum annihilation gives rise to a " line " spectrum of 510 kev radiation while three-quantum decay, characteristic of the triplet state, yields a continuous gamma ray spectrum. A measurement of the relative intensity of the continuum and the 510 kev line therefore indicates the probability of annihilation in a triplet state. Such a measurement is relatively easy by means of a scintillation gamma-ray spectrometer, and this is the method adopted in our experiments. Other methods have been employed by other workers (Pond 1952, Wheatley and Halliday 1952).

After establishing the abundant formation of positronium in gases and after preliminary studies to establish favorable conditions for the experiments, we proceeded to investigate the fine structure of the ground state. To understand these experiments, consider the Zeeman effect of the ^1S and ^3S levels as illustrated in Figure 2. It is a remarkable property of positronium that none of its states shows a linear Zeeman effect, i.e. none of them has a permanent magnetic moment. This is rather easy to understand in terms of a semi-classical model: since any orbit is equally traversed by a negatron and a positron, there is no net current associated with the orbital motion and therefore no orbital magnetic moment. The spin magnetic moments have opposite signs with respect to the spin angular momentum for positron and negatron. Thus the moments cancel in triplet states. In singlet states, which are characterized in this picture by an " antiparallel " orientation of the spins, one might expect a net magnetic moment since the two moments should be parallel. However, since there is no preferred direction in this state, the moment will fluctuate rapidly and its average observed value is again zero.

While positronium has no permanent magnetic moment, there may be an induced moment in a magnetic field and therefore a second order Zeeman effect. Quantum mechanically this is due to the " mixing " of states with equal quantum numbers L and m_s but different total spin S. The sub-levels with $m = \pm 1$ of the triplet state are completely unaffected by the magnetic field since there are no $^1S_0$ components of the same m with which they could combine.
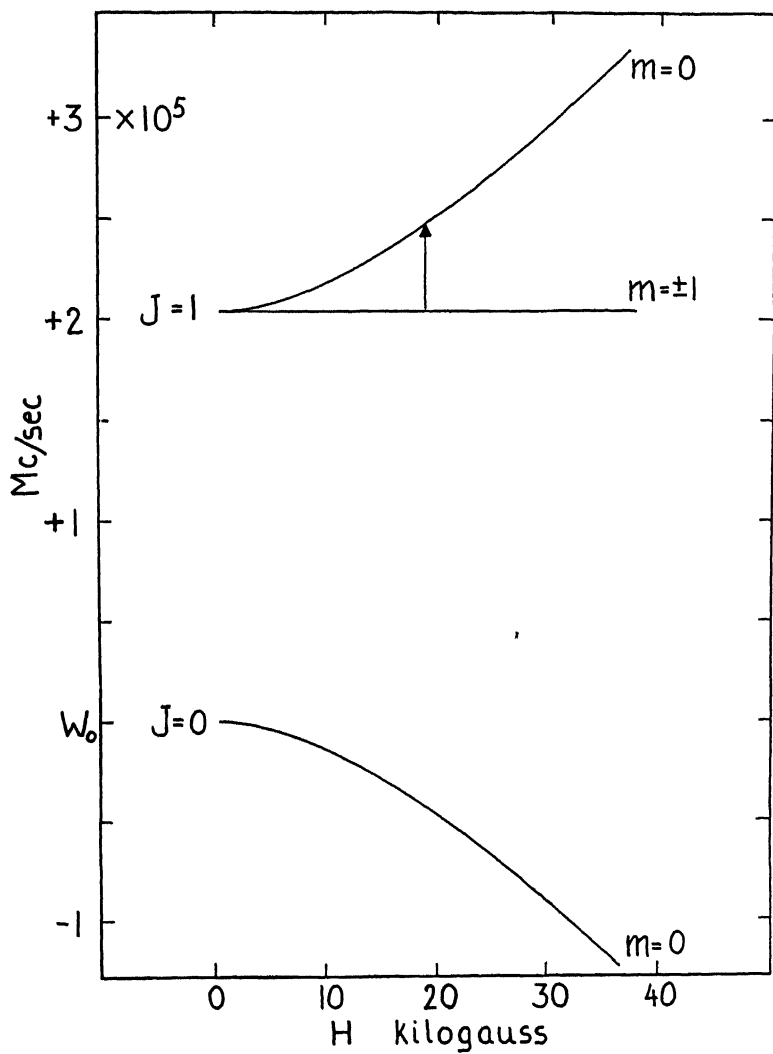
FIGURE 2

On the other hand the sub-state with $J = 1$, $m = 0$ can mix with the singlet level. In a very strong field two eigen-states of the system can be described by stating that in one of them the positron spin points in the direction of the magnetic field and the negatron spin in the opposite direction, while in the other state these directions are reversed. Each of these states is a mixture of equal contributions from the singlet and triplet components with $m = 0$. In this high-field (Paschen-Back) region the magnetic energy is proportional to $2\mu H$. In low fields the admixture of singlet to the triplet wave function (or vice versa) is given by

$$a = (x/2) \, (1 + x^2/4)^{-1/2}, \, x = 4\mu H/\Delta W \qquad (5)$$

ΔW is the singlet-triplet splitting (Equation 1). The magnetic energy w is approximately

$$w = \pm 2\mu Ha = (2\mu H)^2/\Delta W; \, a << 1 \qquad (6)$$

i.e. the Zeeman effect is quadratic. As we see in Figure 2, the sign of the effect is such that the two states " repel " each other as is usual with second order perturbations. A more accurate expression for w can be obtained without resort to perturbation procedures. The expression is essentially the Breit-Rabi formula. The energies of the levels with respect to the unperturbed singlet state are

$$W - W_0 = (\Delta W/2)\left[1 = mx \pm (1 - 2mx + x^2)^{1/2} \right] \qquad (7)$$

(c.f. Figure 2). One of the consequences of the application of a magnetic field is the admixture of a certain amount of singlet wave function to the triplet state with $m_s = 0$. One may say that the atoms, which in the absence of the field were in this state, now spend a fraction of the time $a^2 = (2\mu H/\Delta W)^2$ as a singlet. But the singlet state can be annihilated by two-quantum emission at a rate 1100 times faster than the decay of the triplet. Thus, in a very strong magnetic field all of the $m_s = 0$ atoms (one third of the ortho-positronium) will decay by two-quantum instead of three-quantum annihilation. In weaker fields the three-quantum emission is partly quenched, the fraction ϕ remaining in a field H being

$\phi = \dfrac{\lambda_0/\lambda_p + a^2(1 - 1/3)}{a^2 + \lambda_0/\lambda_p}$. The factor $1/3$ is modified somewhat

for some methods of observation by the angular distribution of the radiations with respect to the direction of the magnetic field.

Deutsch and Dulit (1951) observed ϕ as a function of H. Assuming λ_0/λ_p to be given by Equation 4, we used ϕ to determine a^2 and, from this, ΔW. Although the determination was rather rough, it showed clearly the existence of the " annihilation force " term. Wheatley and Halliday (1952) repeated these experiments with slightly better accuracy. Assuming ΔW to have the value determined by Deutsch and Brown in an experiment to be described below, they used their results to calculate λ_0/λ_p. Good agreement with Equation 4 was found. Taken together with the experimental determination of λ_0 by Deutsch (1951), this work of Wheatley and Halliday also establishes an experimental verification of Equation 3.

While these experiments determined ΔW with an accuracy of about $\pm 10\%$, it seemed desirable to measure this quantity with high precision. The obvious procedure would be to induce transitions from the triplet to the singlet state by means of a radio-frequency field and to observe the resulting reduction in the three-quantum decay. Unfortunately the required high radio-frequency fields (several gauss) are at present not available at the frequency of this transition (2×10^5 Mc/sec). We therefore decided to measure instead transitions between the $m = \pm 1$ and $m = 0$ components of the triplet, indicated by the arrow in Figure 2. The frequency of this transition can be calculated from Equation 7 and a suitable choice of H brings it into the range of the available source of radio-frequency power. In a field of about 10,000 gauss the transition frequency is about 3,000 Mc/sec. The experiment was designed as follows: A positron emitting source was deposited at one end of a resonant cavity located in a uniform magnetic field of about 10^4 gauss. The cavity was excited by a magnetron in a suitable mode. A scintillation spectrometer analyzed the gamma ray spectrum arising from the positrons annihilated in the gas filling the cavity, while the magnetic field was varied in the neighborhood of the resonance value given by Equation 7. In a magnetic field of this magnitude the $m = 0$ state decays almost entirely by two-quantum annihilation, while the $m = \pm 1$ states decay entirely by emission of three quanta. Thus, at resonance, one expects to observe a reduction of the continuous gamma ray spectrum compared with the 510 kev line.

The results of Deutsch and Brown (1952) did indeed show this
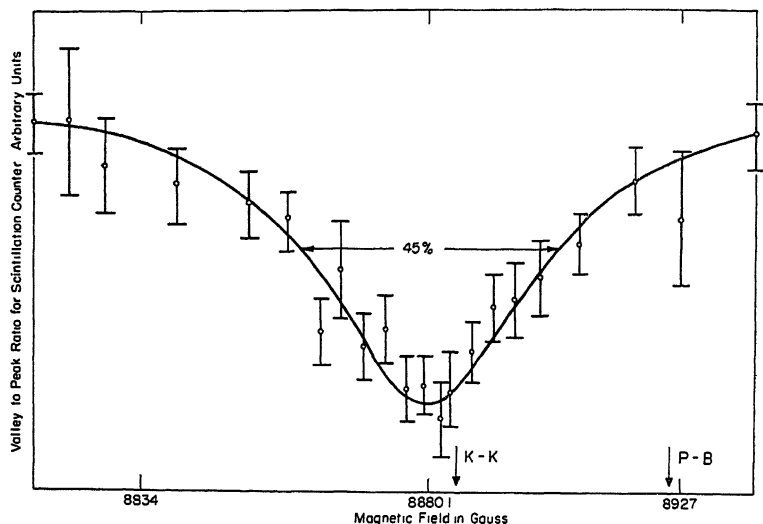
FIGURE 3

phenomenon and yielded a measurement of the ground state split-
ting, ΔW. Figure 3 shows a typical curve, more recent than the
published results of Deutsch and Brown, of the three-quantum
intensity, in arbitrary units as a function of the magnetic field near
the resonance, with a radio-frequency field of about 3,000 Mc/sec.
The arrow marked *P-B* shows the position of the resonance ex-
pected from Equation 1, in clear disagreement with the experi-
mental result.

Equation 1 holds only to order a^2 with respect to the gross
structure. Karplus and Klein have calculated the splitting to
the order a^3. The corrections involved arise, e.g., from the
effect of the "anomalous" magnetic moment of the electron
$\mu_e = \mu_0 \, (1 + a/2\pi)$ on the magnetic interaction, from virtual two-
quantum annihilation in the singlet state, from zero-point fluctua-
tions of the electromagnetic field, etc. As a result Equation 1 is
replaced by

$$\Delta W = (\mu^2_0/a^3_0)\left[7/3 - (32/9 + 2 \ln 2)a/\pi \right] = 2.0337 \text{ Mc/sec. (8)}$$

The value of the resonance magnetic field derived from Equation
8 is indicated in Figure 3 by the arrow marked *K-K* and is in

satisfactory agreement with experiment. The small remaining discrepancy is within the experimental and theoretical uncertainties. Experiments are now under way to increase the precision of the measurements. In the meantime we can conclude that the predictions of the new quantum electrodynamics are in agreement with the experimental results concerning the interaction between electrons — an agreement within the already quite respectable accuracy of the experiments.

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Excited Hydrogen Atoms

W. E. Lamb, Jr.
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One might divide spectroscopic methods into two classes. In the first, the physical substance of interest emits light which is examined in a spectroscope, or else modifies radiation which passes through it. In the second, some detectable property of the matter, or radiation from it, is changed appreciably by subjecting the sample to an electromagnetic field of relatively low frequency.

Examples of the first class include: emission, absorption and Raman spectroscopy, nuclear induction, and conventional microwave spectroscopy. Methods of the second category do not have almost universal applicability as do those of the first, but when appropriate, may afford resolution not otherwise attainable. Three of the four papers this afternoon deal with methods of the second class. Even in Purcell's report on radio astronomy the hydrogen hyperfine transition was first discovered with an atomic beam, one of the methods of the second class. In Deutsch's work on positronium a static or microwave magnetic field can trigger the emission of half million volt gamma rays.

I shall describe briefly the application of methods of the second type to a study of energy levels of the hydrogen atom. For simplicity of discussion I consider an idealized hydrogen atom in which the electron has no spin or magnetic moment, and relativity effects may be neglected. A consequence of the Coulomb attraction is that levels with the same principal quantum number n and differing azimuthal quantum numbers l should coincide in energy exactly. In actuality, the coincidence is not exact, and the frequency separations lie in the microwave and radio-frequency spectrum.

If we excite hydrogen atoms by electron bombardment, both levels ns and np will be populated. In general, the level ns is much longer lived than the corresponding level np. (For $n = 2$, $2s$ is metastable and has a life of a tenth of a second if the atom is isolated, while the life of $2p$ is about 10^{-9} second.) The life of $3s$ is about 0.1 micro-second and is 30 times longer than that of $3p$. The

cross-section is somewhat larger for the p level, but the longer life of s outweighs this, and one therefore expects to have a larger steady state population of ns than of np produced by the electron bombardment. The state $2p$ can radiate the Lyman-alpha ultraviolet radiation while $2s$ cannot because of selection rules. Hence if we cause transitions $2s$-$2p$ by r.f. fields of frequency corresponding to the energy separation of the states, we get an increase of intensity of Lyman-alpha or alternately a decrease in the number of metastable atoms. The former circumstance was used by Miriam Skinner and myself in a determination of the fine structure of singly ionized helium, and the latter by Retherford, Dayhoff and Triebwasser for a study of atomic hydrogen and deuterium.

The levels $n = 3$ can radiate both the Lyman-beta and H-alpha radiation of the Balmer series. While the level $3s$ can emit just the latter, only 10% of the $3p$ atoms can give the former because of the competing emission of Lyman radiation. The intensity of H-alpha should be decreased when radiowaves of the right frequency are applied to the excited atoms and cause transitions between $3s$ and $3p$. At Stanford University, Maiman and I are attempting a determination of the $n = 3$ fine structure in this way. Calculations of expected effects and preliminary observations are very encouraging.

Fairly obvious extensions of method should permit the study of higher levels of hydrogen and singly ionized helium. The only limit is sensitivity of the detector of the radiation and the amount of background light, which can usually be much reduced by a suitable filter.

When results of such measurements are compared with theory one obtains a test of the Dirac relativistic electron theory as well as the Coulomb inverse square law of attraction between nucleus and electron. For instance, in deuterium $2s$ was observed to be higher than $2p$ by 1059.0 Mc/sec. Most of this discrepancy has been explained by theoretical developments since 1947. About 1058.3 Mc/sec is caused by the interaction of the electron with the quantum mechanical vacuum (zero point oscillations of the electromagnetic field, and the infinite sea of negative energy electrons). This factor effectively smears out the point electron into a sphere of radius 7×10^{-12} cm, and in addition gives the electron an anomalous magnetic moment. About 0.75 Mc/sec is contributed by the

finite extension of the deuteron, while -0.6 Mc/sec is due to nuclear recoil effects.

When all such computations are added up there still seems to be a discrepancy of 0.5 Mc/sec, with claimed accuracy of about 0.1 Mc/sec for both experiment and calculations. The explanation is not known at present, but is probably to be found in some higher order quantum electrodynamic effect or in a short-range electron-proton interaction.

For distances of several feet or so, Coulomb's law has been tested very accurately by Faraday Ice Pail experiments. Suppose we were to consider a change from $1/r^2$ to $1/r^{2\pm\epsilon}$. From the negative result of such experiments, Maxwell was able to show that $\epsilon < 1/(21,600)$, while in 1936 Plimpton and Lawton found $\epsilon < 2 \times 10^{-9}$.

It is amusing to consider what value of ϵ would arise if we were to interpret the discrepancy of 0.5 Mc/sec as arising from such a change in the law of force for the much smaller distances of order 10^{-8} cm and less. One finds $\epsilon = 1 \times 10^{-8}$. In this sense then the microwave measurements of the hydrogen spectrum may be regarded as " Faraday Ice Pail " experiments, on a very small scale, in which a positive effect is found. However, as indicated earlier, it is certainly not claimed that any departure from Coulomb's law can be described by a simple change of the exponent from two.

Line Spectra in Radio Astronomy

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Over the range of wavelength from one centimeter to ten meters, roughly speaking, the earth's atmosphere is practically transparent to electromagnetic radiation. The long wavelength limit of this " radio window " is imposed by the ionosphere. At the short wavelength end, selective absorption by oxygen and water vapor is encountered, though bands of partial transparency may be found down to wavelengths of a millimeter or less. Most of the radio energy that reaches the earth from the outside, whether from the sun or more remote sources, is distributed continuously over the spectrum, like white light, but with an intensity that increases strongly toward *longer* wavelengths. It is convenient to describe the intensity of the radiation at a particular wavelength by giving the temperature of a cavity within which one would find the same flux of radiation at that particular wavelength. In the microwave band, the incoming radiation has so low an intensity that its " equivalent radiation temperature " is at most 10 or 20 degrees Kelvin. What radiation there is appears to come mainly from our own galaxy.

The possible existence of a characteristic spectral line in this weak continuum was suggested by the Dutch astrophysicist H. C. van de Hulst. A substantial fraction of the matter in our galaxy is atomic hydrogen, sparsely distributed as an interstellar gas. The ground level of the hydrogen atom is actually double, owing to the interaction of the magnetic moments of the electron and the hydrogen nucleus. This " hyperfine " splitting corresponds to a frequency of 1420 Mc/sec, so that the hydrogen atom in its ground state is capable of absorbing or emitting quanta of this frequency. The presence of this " 21 centimeter " line was first detected about two years ago by H. I. Ewen at Harvard. Very soon thereafter it was observed by Oort and Muller in Leiden, and by radioastronomers in Australia.

The method of observation is simple in principle. A microwave antenna is directed at the Milky Way and the energy received in

one narrow spectral band is carefully compared with that received in some nearby spectral band; the spectrum is slowly explored in this way, until one finds a narrow band in which the intensity is significantly different from that in the neighboring parts of the spectrum. The hydrogen line appears in emission; that is, the intensity of radiation at this frequency is higher than that of the continuum. The difference in equivalent radiation temperature between line and continuum is only some 50 degrees, corresponding to about 10^{-17} watts change in the input to the receiver. (The hydrogen 21-cm radiation falling on the entire earth amounts to about one watt!) Still, the refinement of radio techniques permits one to measure not only the intensity of the line, but its shape or " profile," and, of course, its frequency. Already much has been learned about the distribution of hydrogen in the galaxy and the conditions within the tenuous clouds of otherwise quite invisible interstellar gas. For example, the measured line intensity is closely related to the actual kinetic temperature of the hydrogen gas, which in this way is found to be some 50 to 100 degrees Kelvin.

Are there sources of line emission other than the hydrogen clouds? Are there other atomic or molecular transitions that might be found in the radio spectrum of astronomical sources? One thinks first of the sun, and here one meets a serious obstacle. The outer solar envelope is an extremely hot gas, almost totally ionized. It strongly emits *and strongly absorbs* radio waves. The cooler interior layers near the photosphere, where unionized atoms can exist, are screened from us by this opaque blanket of ionization. There does not seem to be much hope of finding the 21-cm line in the sun's radio spectrum; Oort and Muller in fact looked for it without success. There is another interesting microwave line from hydrogen, the transition $2S_{1/2} - 2P_{3/2}$ in the first excited state. This is one of the transitions involved in Professor Lamb's experiments. Were it not for the circumstance just mentioned, this line (wavelength about 3 cm) ought to appear in the sun's spectrum, and perhaps under special conditions it might still be found. As for stars other than the sun, one must remember that the resolving power of a radio telescope is so poor that a single remote star can make only a relatively tiny contribution to the power entering the receiver at one time. Except for local objects, one must think in terms of extended, or at any rate multiple, sources.

Returning to the interstellar medium, the abundance of atoms other than hydrogen is of course very small. Deuterium has a hyperfine transition, the detection of which does not seem entirely hopeless. It has been suggested by the Russian astrophysicist Shklovsky that certain of the *hydrides*, in particular OH and CH, which are believed to be present in the interstellar gas, have lines in the microwave spectrum. These lines arise from the so-called Λ-type doublets of the ground state of the molecule. In the case of OH, this transition has recently been detected in the laboratory by Townes at Columbia, thus giving the radio astronomer the important advantage of knowing just where to look in the spectrum.

The microwave spectroscopists have, to be sure, catalogued a vast number of molecular rotational lines throughout the centimeter-wavelength range. But to have its pure rotational transitions fall in the centimeter range, a molecule must have two heavy ends, so to speak. Such molecules are very rare in a gas of which the overwhelmingly predominant constituent is hydrogen, at a total pressure of 10^{-19} atmospheres! Perhaps the most promising candidates are the heavy hydrides such as CaH, for which the first rotational transition occurs at a wavelength a little under one millimeter. But before radio astronomy can move into this range there will have to be some advance in microwave techniques. And we shall have to explore the atmospheric absorption to see if we can in fact find a reasonably transparent "window" through which to look out.

Summary of Communication by Enrico Fermi on Meson Physics

WALTER SELOVE
Harvard University

Dr. Fermi discussed the development of meson physics, which was started by Yukawa's original theoretical suggestion in 1935 that the short-range forces between nucleons could be interpreted in terms of a field whose particles, the mesons, have a mass related to the range of the forces. The particles of approximately correct mass discovered shortly afterwards in cosmic radiation were shown some ten years later to consist of " mu "-mesons, which do not interact strongly with nucleons. There are, however, "pi"-mesons, which do seem to fit the specifications of Yukawa's theory, and most meson research at present is directed at their properties and interactions with nuclear matter.

The pi-mesons were also originally discovered in cosmic radiation. Cosmic radiation still is the only source of the highest energies. (Fermi pointed out that even the present accelerators, which produce particles with energies measured in billions of electron volts, cannot compete with the 10^{16} electron volt energies found in cosmic rays; to obtain such energies with present methods one would need a ring of satellites with a field of 10,000 gauss all around the earth — not a simple operation, but at least one would have no difficulty in obtaining the necessary vacuum!) Within their energy range, however, present accelerators furnish mesons in enormously greater numbers than those from cosmic rays; for example, at Chicago the 450 million volt proton beam produces a very respectable meson source of 10^{10} to 10^{11} mesons per second.

What kind of information can one hope and try to get about mesons? As for any new particle (and very many new ones are currently being discovered) one first asks questions on the particle itself — its charge, mass, spin, parity, etc. The charge ($+ 1, -1$, and zero are known), spin (zero), and parity (odd) have been determined through experiments, and the mass is at present known for the various types to about 1%. The next kind of investigation

we try to carry out with the mesons, following again a fairly well established pattern with regard to new particles, is to scatter them against various other particles and try to interpret the results in terms of the forces that operate between the mesons and the other particles. The possible kinds of scattering experiments are limited by the type of particles we can have in reasonably abundant concentration — e.g., it would be a rather unfair question to ask for a direct measurement of the scattering between neutrons and mesons because we cannot very well obtain a container of either neutrons or mesons with which to carry out the experiment. There is, however, a further and even more fundamental limitation to the information we can obtain in such scattering experiments. The interactions of the fundamental particles with the fields are so strong that each object that we call an elementary particle does not consist of an ideal mathematical point object, but instead it has a diffuse complicated structure, covering a region with a diameter of the order of 10^{-13} cm. A scattering between two such particles can thus be regarded as a kind of hydrodynamical collision, the results of which do not give us very direct information about the interactions of the ideal mathematical particles. But, we do get some information by such experiments, and this limited type of information is as much as we can hope to acquire by the experimentation that is in sight at present.

The present scattering experiments can be analyzed to give the dependence on angular momentum, on isotopic spin, and on energy. The experimental data is rather crude, because the scattering cross sections involved are so small that counting rates are very low. Within this limitation, the experimental data obtained so far in the scattering of negative and positive pi-mesons by protons may be described in a self-consistent way within the present framework of scattering theory, but it is too early yet to draw very detailed conclusions about the nature of the interactions.

Summary of Communication
by J. Robert Oppenheimer on
Recent Progress in the Understanding of
Nuclear Forces

WALTER SELOVE

Harvard University

Dr. Oppenheimer spoke on recent progress in understanding nuclear forces. We now know a good deal more about nuclear forces than we knew as recently as five to seven years ago, although the theory is still in a rather crude state. One might say that the three most important factors involved in an understanding of nuclear forces are (1) experimental information, (2) general principles, and (3) good ideas. With regard to the first of these, much new information has become available because of the higher energies available since the war. In the area of nucleon-nucleon interactions at moderate energies, these experiments have shown the neutron-proton interaction to give scattering which is strongly predominant in the forward and backward directions, thus showing a small probability of high momentum transfer, whereas the proton-proton interaction shows a much " harder " nature, with scattering at high energies being essentially insensitive to angle and energy.

With regard to the second factor, there has been new evidence that nucleon-nucleon interactions are charge-independent, i.e., that neutron-neutron, proton-proton, and neutron-proton forces are equal. This principle is more puzzling than any other general invariance or conservation law, especially so because its fundamental explanation is unclear and because it seems to be only approximately true rather than rigorously.

As for new fundamental ideas, there have really been none since Yukawa's original one. Recent theoretical work has consisted of attempts to work out the details of a meson theory in the face of the difficulties mentioned below. Meson theories have involved difficult and uncertain calculations from the start, and although the

discovery of pi-mesons gave new life to the Yukawa hypothesis, one still has several misgivings about the present form of meson theory. For one thing, the fact that each " physical " particle presumably consists of a diffuse and complicated structure involving more than just a single " ideal " particle makes it very difficult to analyze experimental interactions in terms of " ideal " particles. Secondly, although meson theories can be made to give some agreement with experiment, agreement can be obtained by means of various, mutually inconsistent, theories. And third, and most important, the present form of meson theory has a logically unsatisfactory basis. It is based on a weak-coupling type calculation, whereas in fact the interaction between nucleons and mesons is known to be strong.

This last difficulty may be compared with the situation in electromagnetic theory. First, as to the magnitude of the interaction constant — the number which measures the extent to which the " real " particle differs from the " ideal " particle, measures the extent to which interaction between the particle and the field complicates the situation. For the interaction between the electron and the radiation field, the interaction constant is about 0.01. For the quantum theory of the meson field, the corresponding number is of the order of 10. Now the interaction can be formulated in terms of a power series expansion in powers of the coupling constant. For the electromagnetic field one may hope that the successive terms in such a series will grow rapidly much smaller. Even for this field, however, there were serious difficulties in obtaining such a convergent type of result, until the recent development by Schwinger, Tomonaga, Feynman, and Dyson, on " re-normalization " of charge and mass. Since their work, the theory of quantum electrodynamics has been in a fairly satisfactory state. However, in the case of meson theory, an expansion in terms of a coupling constant of order 10 would certainly not be satisfactory. Actually, it appears that it may be possible to formulate the theory so as to involve an expansion not in powers of this coupling constant, but in the product of it with the square of the ratio of the meson and nucleon masses, a number about .01.

The big question in the present state of the theory is whether or not one can obtain a correct expansion solely in this smaller product parameter. It is not certain whether within this " weak-

coupling " framework there are convergent solutions at all. In any event probably the most one can hope for from this approach is a rough approximation to a correct description of nuclear forces.

Dyson has shown how one might carry out re-normalization in the meson field problem, and how one might hope to obtain a theory having perhaps some accuracy in the low-energy region. This approach is now being primarily used in the meson-nucleon scattering problem. A somewhat similar idea has been pursued by Levy in investigating the nucleon-nucleon interaction. He unfortunately made some serious errors in his work, but his results may nevertheless be important because of the good agreement obtained by comparing the results of his calculated nucleon-nucleon potential with experimental data on the properties of the neutron-proton and proton-proton systems at energies up to about 40 Mev. Many further calculations are being made along the lines of his approach. They give various forms for the interaction potential, although they all seem to have in common the short-range repulsive feature indicated by Levy. Results are not yet complete as to whether these newer attempts can actually give good agreement with experiment. It is at present not clear whether a theory along the lines of Yukawa's original idea can be made to work within the present formulation, or whether it will be necessary to introduce the very deep reforms in the whole description of the structure and interaction of elementary particles which we know must eventually be made.

Nuclear Spectroscopy

MANNE SIEGBAHN
Nobel Institutet för Fysik

The lecture gave a condensed summary of *experimental results and methods in nuclear spectroscopy developed at the Nobel Institute of Physics in Stockholm*, illustrated with several slides of the spectrometers and other instruments used in these investigations. By refinements in the design of the spectrometers, and improvements in the experimental techniques, the resolving power and the accuracy have been highly increased. Whereas a few years ago a precision of a few per cent was considered satisfactory, the accuracy now obtained is of the order of a few parts in 10,000. Such an increase in accuracy has in this case, as is the general experience in other fields of experimental physics, resulted in revealing many new facts.

For absolute determinations of β-γ energies the classical semicircular type of β-spectrometer is best suited. The new resonance method of Purcell has been used to measure the strength of the magnetic field, as well as to correct for the deviation from homogeneity in the field.

A spectrometer of the same type but using permanent magnets has been developed with the object of getting high resolving power. For this purpose it is necessary to have the spectrometer at constant temperature. A variation of one degree centigrade means a change of 2.2 parts in 10,000 in the magnetic field, which is of the same order of magnitude as some of the effects found and studied.

In the double-focusing spectrometer which was designed and first built in 1946, and later developed into a high precision instrument, the magnetic field is not homogeneous but varies as $1/\sqrt{r}$. In that case one gets a focussed true image in natural size of the β-radiating source at an angle of $\sim 255°$ from the object.

In addition to these three spectrometers with flat magnetic fields, some spectrometers of the lens type have been used in our investigations.

In one of these the β-rays are first collected in *an intermediate ring-shaped image* and then focussed on the G.M.-tube. The desired gradient in the magnetic field is obtained by adjusting the current in the different magnetizing coils. In this way high intensity can be combined with good resolving power. By the use of an adjustable ring-shaped screen in the intermediate focus, the resolving power can be varied within certain limits.

Finally we mention a new type of instrument, a " spectrogoniometer," where a combination of two lens spectrometers is used in the study of coincidence and angular correlations. The two spectrometers can be set at different angles to each other; the radiating source is placed in the center of rotation.

From the results we obtain a list of energy standards for use in β-spectroscopy. These standards cover at present a region of energies from 238 kev to 2753 kev (see Table I) with an accuracy of a few parts in 10,000. In the few cases where measurements have been carried out in other laboratories, especially with the crystal method, at DuMond's Institute in Pasadena, the agreement is now quite satisfactory.

TABLE I

Line	H_ρ Gauss cm	E kev	Method
ThB F	1388.56±0.15	238.63±0.04	absolute
ThB I	1754.01±0.20	238.62±0.04	absolute
Au^{108} K conv.	2222.4 ±0.4	411.75±0.10	against ThC"L
ThC" L	2607.17±0.30	510.85±0.08	absolute
Cs^{137} K conv.	3381.28±0.5	661.65±0.15	absolute, against ThC"L, against annihilation rad.
RaC' R	4839.8 ±0.8	1120.4 ±0.2	absolute
Co^{60} I		1172.8 ±0.5	against Co^{60} II
Co^{60} II		1332.5 ±0.3	against RaC'T
RaC' T	5874.4 ±0.6	1415.8 ±0.2	absolute
ThC" X	9986.7 ±1.5	2614.3 ±0.5	absolute
Na^{24}U$_K$	10363 ±4	2753.2 ±1.0	against ThC"X

As examples of what can be gained by the increased resolving power of the spectrometers, there is shown in Figure 1 spectrograms of some lines in the spectra of Th B + C + C". The F, G, and H-lines, which originate from the K-shell, are definitely broader than the I and I_a-lines belonging to the L_I and L_{II} shells,
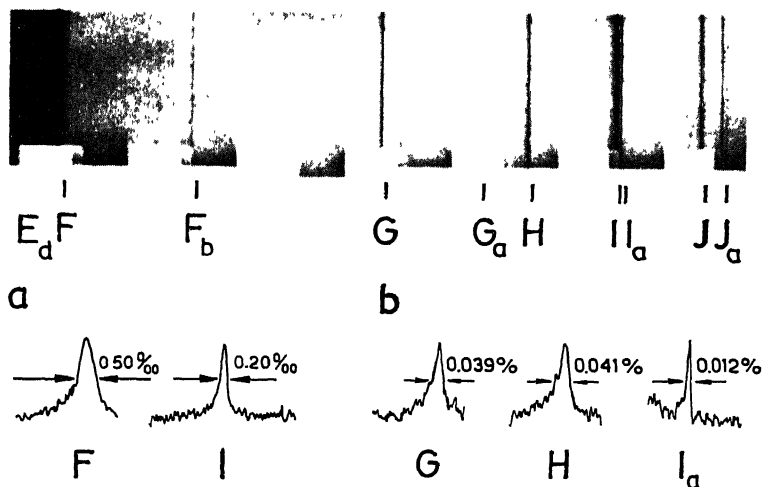
FIGURE 1. *Upper half:* β-SPECTROGRAM TAKEN WITH SEMICIRCULAR, PER-MANENT MAGNET SPECTROMETER FOR HIGH RESOLUTION. *Lower half:* PHOTO-REGISTRATIONS FROM DIFFERENT PLATES: a, OF THE LINES F AND I; AND b, OF LINES G, H AND I_a. (SLÄTIS)

respectively, the difference in width being about 3 in 10,000. This effect can be quantitatively explained in terms of the finite widths of the K level, as extrapolated from the X-ray data for lighter elements.

As another example, the successful elucidation of the very complicated internal conversion spectrum of Bi^{206} is shown in Figure 2, taken from an unpublished paper by Dr. Alburger. The results from the successive increases in resolving power are illustrated. By this investigation it has been possible to give a tentative energy level scheme, which fits the observed data. In part this scheme was based on coincidence measurements by using the spectrogoniometer.

A PORTION OF THE Bi^{206} (K) Pb^{206}
INTERNAL CONVERSION SPECTRUM
(D.E. ALBURGER - UNPUBLISHED DATA)

DOUBLE - FOCUSING SPECTROMETER
AT   0.22 % RESOLUTION

K-881

K-895

L-803

M-803

DOUBLE-FOCUSING SPECTROMETER
AT   0.5 %   RESOLUTION

LENS SPECTROMETER (BROOKHAVEN)
AT   1.2 % RESOLUTION

3950   4000   4050   4100   GAUSS- cm
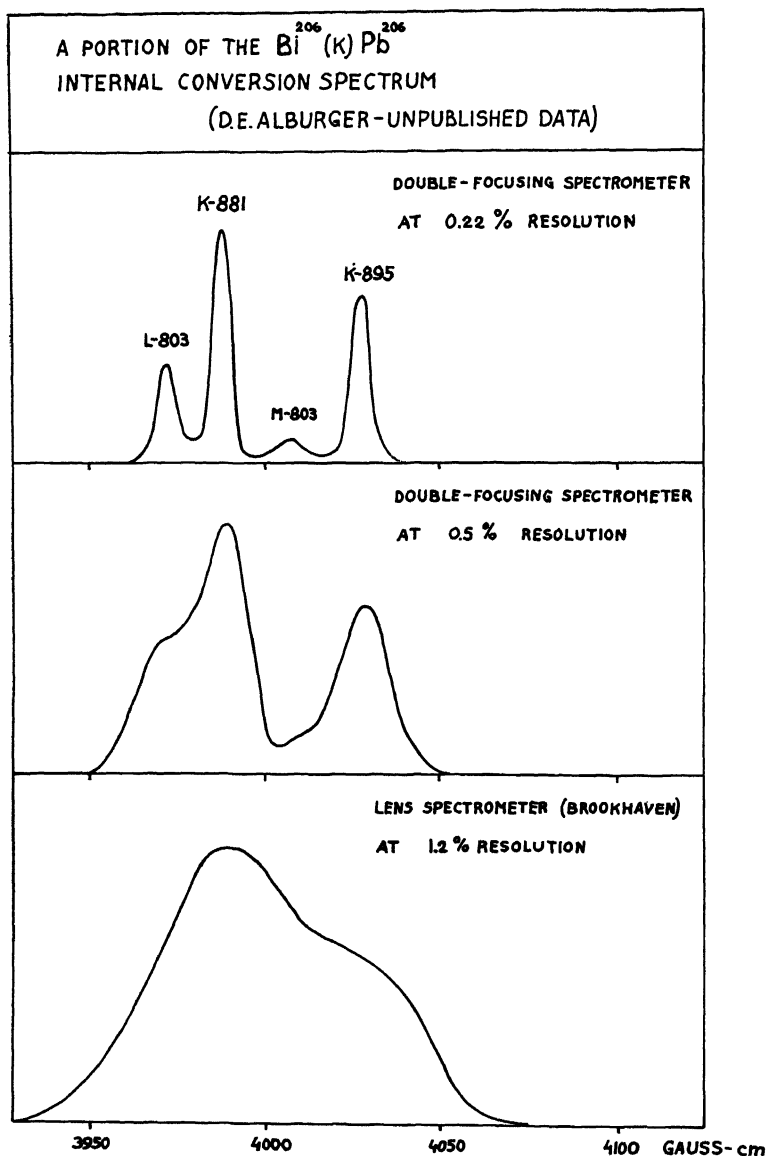
FIGURE 2

The foregoing refers mainly to researches reported in the following papers, and to some newer researches by Dr. Alburger not yet published.

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Phase Transitions in Atomic Nuclei

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Recent advances in our knowledge of the properties of complex nuclei have revealed a number of new and unexpected features. One of the outstanding surprises that have startled nuclear physicists in recent years is the success of the shell model of nuclear structure. This model assumes that the nuclear constituents (nucleons) can be considered to move with very little interaction among each other in a potential well. This potential well represents the overall effect of all other nucleons.

The most unexpected feature of this model is the existence of nucleon orbits within the nucleus with well-defined energy and angular momentum undisturbed by the other nucleons. This is in contradiction to the concept of a strong interaction between nucleons at distances such as those which occur in nuclear matter. No satisfactory explanation has yet been proposed for this phenomenon. Either the nuclear forces are different within nuclear matter compared to the forces between isolated nucleons, or some special mechanism or specially ordered arrangement in the low excited states causes the particles to move as if they were free within a potential well.

Phenomenologically, we can express this fact by the introduction of a " mean free path " l in nuclear matter for the exchange of energy or momentum with other nucleons. The success of the shell model implies that this mean free path l is long compared to the nuclear radius for low excitation energies. The question arises whether l changes with higher excitation energies. There are a number of facts which lead one to believe that l becomes appreciably shorter for higher excitation. The large amount of experimental material concerning nuclear reactions suggests that a compound nucleus is formed after the bombarding of a nucleus with nucleons of several Mev. This means that these projectiles have a mean free path smaller than the nuclear radius. The reaction cross section (cross section for energy exchange) for

neutrons at 15 Mev actually is rather accurately equal to the geometrical cross section ($\pi(R + \lambda)^2$, λ being the deBroglie wavelength). This indicates compound-nucleus formation for every hit and thus $l < < R$. Recent investigations of the total neutron cross section between 0 and 3 Mev indicate that in that region $l \sim 3R$.[1] At very high energy (50 Mev and above), the observed interaction cross section between isolated nucleons becomes small. This gives rise to an increase[2] of l towards $l \sim R$. At these high energies, however, the large l is easily understood from the decrease of the n-p and p-p cross section, whereas the large l at very low energies is yet unexplained. We concentrate our attention on the change in character of the nucleon-nucleus interaction from weak to strong, which seems to take place somewhere at an excitation energy of the order of 8 or 10 Mev.

A similar change can be observed in the level density. Spectroscopic results and inelastic scattering show that the level spectrum of complex nuclei is of the character shown in Figure 1. It has
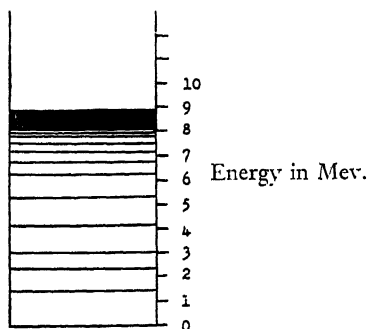
FIGURE 1. TYPICAL LEVEL SPECTRUM OF A COMPLEX NUCLEUS.

large spacings and is similar to a single particle spectrum (shell model) at lower energies, perhaps up to 5-7 Mev. We know from neutron capture experiments, however, that at 8 Mev and higher the levels are very dense. In nuclei with $A > 100$ the level distance is of the order of 30 ev, that is, the levels are about 10^4 times denser than in the lower region. There seems to be a qualitative difference at low and at high excitation which one can interpret as the

transition from the lower well-ordered states of independent particle motion to the higher states with strong interaction in which the motion is complicated and shared by all constituents. Hence the energy dependence of the level density also reproduces the two characteristic nuclear behavior patterns: At low energies the motion is relatively simple, each nucleon moves in an apparent potential field, the stationary states are few and widely spaced. At higher excitation the motion is much more complicated, collective modes of motion are possible, the nucleons exchange energy and momentum, the energy is thoroughly mixed among the constituents, the levels are many and narrowly spaced.

The sharing of the energy at higher excitation among many constituents is well illustrated by the energy distribution of inelastically scattered neutrons of 14 Mev.[3] The secondary neutrons all have an energy much smaller than the primary energy (about 1 or 2 Mev). This means that the energy very probably is distributed over many partners in the nucleus. The spectrum of the emerging neutrons can be compared to the energy distribution in an evaporation. Actually one can derive a temperature from the observed energy distributions, and this temperature was found to
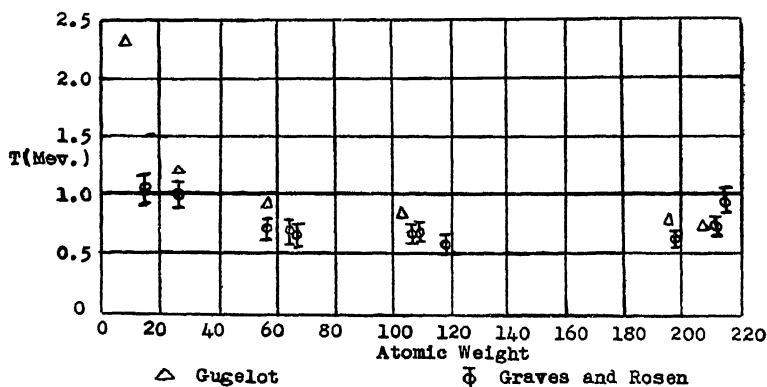
FIGURE 2. VARIATION OF TEMPERATURE WITH MASS NUMBER.

be quite low (~0.75 Mev) for most nuclei. Figure 2 shows the temperatures of the outgoing neutrons observed after bombarding with 16 Mev protons[4] and 14 Mev neutrons.

In terms of the evaporation model it is somewhat surprising that the temperatures of all nuclei above $A \sim 30$ are found roughly equal. One would think that the same energy (15 Mev) would heat up lighter nuclei to a higher temperature than heavier ones simply on the basis of equipartition of energy.

An understanding of this fact is perhaps suggested by the use of simple thermodynamic concepts, whose application to nuclear problems, however, is highly questionable. The two types of states in nuclear matter which we encountered at low and high excitation might perhaps be interpreted as two " phases " of nuclear matter; one ordered phase of low entropy and low level density and another phase of higher entropy and level density, which corresponds to the statistical motion. A certain amount of latent heat is necessary to " melt " the first phase into the second. The 15 Mev which are delivered to the nucleus are probably sufficient to " melt " a nucleus smaller than $A \sim 50$. Hence, the temperature which is obtained rises with decreasing A for $A < 50$. If $A > 50$, the energy is sufficient to " melt " only part of the nucleus. Hence, the temperature that is reached is independent of A and is equal to the " melting " temperature (probably ~ 0.75 Mev). Even at the highest A the energy is enough to melt part of the nucleus.

The applicability of this qualitative picture might be tested by further experiments at higher and lower energy. At somewhat higher incident energies the temperatures of the heavier nuclei should be again about equal to the " melting " temperature, but the limiting A for which this occurs should be correspondingly higher. Further experiments will tell whether it is possible to describe the qualitative behavior of the emitted nucleons by applying thermodynamic analogies. Such concepts are sometimes useful for a first orientation and can serve as a valuable guide for the sifting of the complicated experimental material.

The study of the behavior of complex nuclei is of great importance for the fundamental question: How do nuclear particles interact when they occur in large aggregates? We are faced with a quite different problem here than, say, in the structure of solids. In the latter case, we do know the elementary forces acting between atoms, and the problem is entirely a mathematical and methodical one of applying this knowledge to an aggregate of atoms. In the nuclear case, however, the elementary forces be-

tween the constituents are almost completely unknown, since the little knowledge which we have acquired from the study of the forces between two isolated nucleons seems to be inapplicable in the case of densely packed nuclear matter. The question of what keeps neutrons and protons together in atomic nuclei is still largely unanswered.

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INDEX TO VOLUME 82

Proceedings of the American Academy of Arts and Sciences

INDEX TO VOLUME 82

A. INDEX OF PAPERS BY AUTHOR AND BY TITLE

B. Index to Affairs of the American Academy of Arts and Sciences